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PROSPECTUS.

IN a science like that of medicine, where the wisest votary lives only to learn, it would, at first sight, seem superfluous to assign the motives for attempting to reflect light upon any one of its various departments. Bookmaking, however, has become a trade so common, that the world has been led to suspect most literary proposals, as projects to extract money. Under this impression, it becomes necessary, for those actuated by the purest motives, to cover themselves from the shafts of criticism and censure by the fashionable ægis, an apology.

None but a coincidence of circumstances peculiar to myself, could have impelled me to embark in so hazardous an enterprize, as the publication of a Medical and Philosophical Journal in this city. As it is problematical whether the subscription will ever be sufficient to encounter the expense, my efforts can be considered only as an experiment; and the possibility of a failure, must exonerate me from the imputation of pecuniary views. I am exquisitely sensible of the arduous duties the office of an editor imposes, and arrogate

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to myself no pretensions but an ardent zeal to subserve the interests of science and humanity.

When we reflect on the antiquity of the settlement of Maryland, the distinguished rank she has ever maintained in a civil and political view, the population of the city of Baltimore, and the felicity of her situation in the centre of the United States, it becomes a subject of equal admiration and regret, that in literature she has made no figure in the bright constellation of her sister states. In no department of science is this dearth of intelligence more to be deplored, than in that profession to which I have the honor, and perhaps the misfortune to belong. Notwithstanding the widely extended territory of the United States and the vast number of excellent literary institutions so young a country can boast, New-York and Philadelphia only have succeeded in establishing the periodical depositories of medical information. While far the greater portion of the Union affords nothing more permanent than the transitory ephemeris of a news-paper, the southern and middle states exhibit the most ample field for observation, and abound with distinguished philosophers and physicians, whose talents are rusting in obscurity for want of use. To such, it is to be presumed, an invitation only is wanting to excite a spirit of emulation, and to induce them to assert their just claims to literary pre-eminence.

The establishment of a Medical School in this city, the high temperature of the climate, and the shortness of the winters, which must ever preclude the possibility of a complete course of dissection further south, inspire the most sanguine expectation, that the rays of medical light scattered over our southern country, will finally converge in the city of Baltimore.

The relinquishment of the Medical Recorder, by Dr. Watkins, in consequence of his occupations at the Marine Hospital, and a desire of invigorating the spirit of inquiry it had excited, added to the solicitation of some of the most respectable of the faculty in the United States, suggested to me the necessity of a periodical work. At this moment the

utility of such a work is greatly inhanced, by the exorbitant price of imported books. The physician who is determined to move in concert with the improvements of his profession, cannot accomplish his wishes without incuring a heavy expense; more especially when located at a distance from public libraries, only to be found in our most populous cities—Impressed with the importance of this fact, arrangements have been made for procuring from Europe the best periodical works, and a summary of their contents will be exhibited in the Lycæum, as succinctly and promptly as possible.

It will be proper to make some remarks on the more immediate objects of the work. The science of physic is the science of all nature. The physician who confines himself to the dead letter of medicine, cannot be enabled to take a liberal or expanded view of his profession. The sphere of such a miscellany must, therefore, be co-extensive with all the collateral branches of philosophy. Independent of the ordinary topics of medical discussion, there are others deeply interesting, and some of them peculiarly so to an American.

Dissections hold the first place among medical disquisitions. They not only teach us the causes and seats of diseases, but often lead us to the important determination how far the causes are general or local in their operation. Perhaps one of the most important questions in medicine is, whether the causes of diseases be local or general in the first instance: probably they are most of them primarily local. This important question must be determined by dissection and experiment.

Experiments conducted agreeably to the principles of sound logic, must ever interest the medical philosopher. There are many controverted points still left to visionary hypothesis and conjecture, that ought long to have been decided by experiment.

The intimate connection between chemistry and agriculture has hitherto, attracted too little attention, especially in

America. The nature of every soil must be ascertained through the medium of chemistry, for unless the husbandman understand the principle, he is an empyrick, and instead of certainty in his resutls, he is left to chance and the casualties of the elements.

Diseases are as necessarily the offspring of certain soils as the verdure by which they are clothed. The question how far certain diseases are attached to particular soils, as well as the means of obviating or destroying an inquinated atmosphere, are equally the business of the physician, the philosopher, and the citizen.

The strong affinity between chemistry and manufactures, is at this moment a subject of peculiar interest to every patriotic American. The art of *dying*, the most difficult attainment of our most important manufactures, depends essentially upon a knowledge of chemistry.

The animal, vegetable, and mineral kingdoms of our country, are all objects of the first importance to the manufacturer, the naturalist, and the physician. We have yet to learn how far our own country is capable of furnishing the various materials employed in medicine and the arts, for most of which we are still dependant on other nations. From the almost infinite variety of our soil and the progress made of late years in mineralogical researches, it is fair to conclude, that there lies hid in the bosom of our earth the richest abundance.

The variety of *Mineral Springs* that have lately been discovered in our country, especially in Maryland, and the almost irresistible popular current in their favour, render a chemical analysis indispensable. Until their properties shall have been perfectly ascertained, their most transcendent virtues must be used empirically.

The vegetable kingdom, viewed either in its connection with the arts generally or medicine, more particularly holds out the most inviting attractions. Many of our most energetic remedies are derived from it, and the vast unexplored

wilds of our continent, probably offer to the botanist the richest repast now left upon the globe.

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CONDITIONS.

The **LYCÆUM** will be printed quarterly, on fine paper and a neat type at Two Dollars per annum, one dollar to be paid on the delivery of the first number and half a dollar for each subsequent number.

☞ All Communications to be addressed to the Editor, or to Mr **GEORGE HILL**, BOOKSELLER, Baltimore— (postage paid.)

WILLIAMSON

ON SCROFULA.

Baltimore, February 23d, 1811.

MY FRIEND,

ACCORDING to promise I now send you the first part of my essay on Scrofula, &c.

I know of no disease, to which the human system is subject that appears to be less understood and less successfully treated, than Scrofula. It has been my lot to meet with more Scrofulous patients in the course of the last four years than physicians generally do; and it has been my good fortune to be successful in every case that has yet come under my care. I therefore feel myself bound to the faculty in particular, and to my fellow-citizens in general, to communicate publicly my information on the subject. I expected, in the course of the present year, to have published a small volume on this disease with some others that have excited much interest in the medical world, in consequence of their having been very fatal to the human species, but have been prevented from prosecuting these interesting pursuits, by having embarked in an extensive business that requires nearly the whole of my attention as well as my time. Owing to this disappointment, I have resolved to publish them, in their present imperfect state. I know of no better means of doing this than through the medium of some of our periodical

publications, and have chosen the one to be conducted by you for that purpose. I hope with the exertion and aid of your friends, you will be able to give our city a more honorable stand, amongst those of our country, in a scientific view than it has hitherto held.

In treating of the Scrofula, I shall not enter into any speculations relative to its theory, but shall confine myself, principally, to practical remarks. After giving the cases and making some remarks relative to the treatment of the Scrofula, I shall notice the Phthisis Pulmonalis. Some of my remarks on this part of my subject may be considered speculative, they will, nevertheless, I flatter myself, be considered rational and interesting.

The first case of Scrofula that occurred in my practice was in May 1806.

The subject of this case was a negro boy aged eighteen years. The disease was seated in the glands, on each side of the neck. On one side the tumor was as large as a pigeon's egg, on the other as large as a hen's egg. The enlargement of the glands was discovered about three months previous to my being consulted. They were not ulcerated.

It were useless to give a history of the constitution of my patient, as medical authors of eminence have not unfrequently told us what constitutions are most subject to this formidable disease.

My patient's pulse was quick and irritable. He complained a little of nausea in the morning, but had ever, previously, been considered healthy, and his appetite was much impaired.

About the 27th of May, he commenced taking a mixture composed of cremor tartar, als. tart. and tartarized antimony. Medicines of this nature have been spoken of by Ontyd and other ingenious authors, as highly beneficial, previous to the administering of active emetics and cathartics, particularly in diseases of an obstinate nature. Whether the virtue of this preparation consists in its disolving or attenuating property in the primæ viæ, as contended by me, I will

not pretend to say; but my practice warrants me in believing it a grand acquisition in the cure of many obstinate diseases, especially those of a febrile nature. I think it rational to suppose that when the system has been long affected with a disease, in which the primæ viæ participate, that their functions must be considerably impaired. That this is the case in fevers is very obvious, and that it is not in the Scrofula, I believe, no observant practitioner, who has paid any attention to this disease will be bold enough to assert.

It is only in fevers of a protracted nature, that I use this febrifuge as preparatory to more active medicines. I have used it in slow jaundice with a like good effect. In both of these diseases, it seems to prepare the way for the more active medicines that are to succeed. Emetics or cathartics which are administered after such a preparation have, from my observations, a much more salutary effect, in these lingering diseases, than those administered without. Perhaps it may be said, they then more effectually cleanse the primæ viæ. Should this be the case, our doctrine must be received without controversy. For whether the internal medicines, in which we most confide, communicate their influence directly to the stomach and from thence through the system by nervous influence, or pass on and act on the chylopoetic organs, or in any other way is eqaully immaterial, as the stomach is primarily interested; and the better it is prepared for the reception of a remedy, the action of which is to be on, or through it, the greater must be the success of that remedy.

After taking this mixture a few days, an emetic of tartarized antimony was given. The succeeding day, viz. the 1st of June, he took a purge of calomel and jalap; after which the chincona was given for a few days, to strengthen the tone of the stomach, and give fresh vigour to the general system. In a very few days he became quite voracious, and the bark was discontinued for tonics of another order.—In this stage of the disease, when the whole system is labour-

ing under increased irritability, would not bark in large quantities or long continued be pernicious ?

On the 5th of June we began with the following medicine.

R. Sulph. antim. præpt. ʒi.
Pv. Calomel ʒi.
Ex. cicutæ ℈i.
Sap. reent ʒſs.
Syr. simp. M. F. pil xxxvi.

One three times a day.

A strong volatile liniment was applied externally. His diet and drink, such as recommended by our best authors, in this disease. Every thing that seemed to disagree with the stomach, that was difficult to digest, or which would subject to acidity was guarded against. Diet easily digested, and at the same time as nutritious as possible, and such as makes healthy chyle, should be preferred. Are fluids as beneficial in this disease as solids of easy digestion ? Do not an excess of fluids enervate the stomach ; consequently the general system ? They are recommended in inflammatory complaints as increasing the serum of the blood, thereby resolving its inflammatory action. If this is a correct theory, they must of course be pernicious in diseases of an atonic nature. I would by no means be supposed entirely opposed to the use of fluids in this disease, but think we should be cautious to admit none but those of a nutritious or stimulating nature, and then in moderation. His lodging was comfortable and dry ; his cloathing as clean as convenient. A humid impure atmosphere is pernicious to the animal system when in health ; in a state of disease it is more obviously so, and should be strictly guarded against in asthenic diseases, particularly those of a glandular affection. Cleanliness in cloathing, as well as that of the skin, &c. is very conducive to health. Perhaps keeping the skin clean has a greater effect in keeping up a healthy determination to the superficies than is generally imagined ; and cleanliness as regards the cloathing is certainly very necessary to support that of the skin.

June 16th.—The tumours were less, and the symptoms, in every respect flattering.

After the symptoms became very favourable, and the strumous tumours had considerably subsided, our patient imagined nothing was the matter, and for some days discontinued the medicine. When the appearance grew worse, and when I painted to him in the worst imaginable colours the nature of the disease, and what must inevitably be the consequence unless he could be cured and every disposition in the system to relapses completely eradicated; he once more consented to have recourse to medical aid.

In diseases of long standing, it is often necessary to change our remedies frequently, and in none perhaps more so, than where the glandular system is principally concerned; as these affections are generally very obstinate. It is an acknowledged fact that all medicines, in a greater or less degree, lose their effect from long use, or rather that the system becomes less susceptible to their operation.

22d.—We commenced with the murate of Barytes and extract of cicuta in conjunction.

I expected much from this composition; my expectations were founded on the basis of great medical authority. The names of Crawford and Hufeland will ever be remembered with medical veneration; the former of these worthy characters called the attention of the medical world to the use of the Barytes in the Scrofula, and the latter has been an able advocate for it in that disease. Such authority cannot insure success, but it should be sufficient to induce us to confide in what they say, and to give what they recommend from successful practice, and not vague theory, a fair trial.

* R. Muriat Barytes ʒi.
Extract cicut ℈ii.
Aqua font ℥ii.
Syrp. simp. Q. S.

* This Barytes was in a saline state.

We commenceed with fifty drops three times a day.

Externally, a poultice of the crumbs of bread made with a strong solution of the muriat of amonia and extract of cicuta was applied three times a day. His neck was washed every morning with a strong infusion or decoction of bitter herbs and soap. He bathed once a day if convenient. Salt water was preferred.

June 30th.—The general symptoms were very favorable. Was at this period taking pills composed of sulp. antm. præpt. &c. which were stronger than the first.

August.—Tumors decreased and other symptoms were favorable. Was at this time taking the muriat of Barytes, &c.—had increased then to eighty drops, three times in twenty four hours. As an external application the lint. vol.

A short time before he was discharged cured, he conceited he was well, and irritated at my continuing to inforce the medicine and regimen—concluded one day to take a large dose and be done with it. He took ninety drops of the solution of the muriat of Barytes and cicuta at a dose. It occasioned considerable nausea, relaxed the whole system and was very near producing syncope. This alarmed him very much and he took special care afterwards not to take a larger dose than prescribed.

The effect this large dose had on the diseased glands was truly astonishing. It seemed to operate like a charm. The tremors were much softened by it, and discussed in a manner unprecedented. From this fact it appears that the muriat of Barytes, like other powerful medicines, has a much more benign effect, when given in as large a quantity as the system will bear, than when given in small doses. But a cautious physician should always exercise that caution which wisdom dictates. And in administering powerful medicines and those of a doubtful nature, begin with small doses, increasing gradually as symptoms, &c, may justify. By pursuing this plan he hazards nothing, provided he is using the medicine the disease indicates. It is also necessary after having discontinued a medicine for some time, on re-

commencing with it, to use the same precaution. Particular atttention should be paid to the age, constitution, &c. Circumstances relative to the brain, lungs, stomach, &c. should be minutely examined. Some persons stomachs are so exceedingly irritable that scarcely any medicine can be given without exciting violent puking attended with alarming symptoms; others, again, are so acutely sensible as to be convulsed or paralysed on taking almost any medicine, and these disastrous affections are immediately conveyed to the brain or lungs through the nervous influence. Until we become acquainted with a patient's constitution, it it always necessary in giving active medicines, to act with extraordinary precaution. If we discover the person under our care to possess such a constitution as patients under similar circumstances, generally do, it is still necessary to extend our caution, lest some contingent circumstances may occur to thwart our intention or to render such treatment as we are then perscribing pernicious. By administering a medicine injudiciously, alarming symptoms may be excited; such as will deter the physician from a further use of it, or such as to make the patient positively object to its use. I doubt not but that circumstances of this nature are always pregnant with great evil, frequently destroying the reputation of the most invaluable medicines, also, that of *eminent* physicians, and rendering it impossible to cure the patient, whereas had a more judicous practice been observed, neither of these evils would have occured. Be cautious but not fearful, should be a ruling maxim with a physician. And while he is sedulously engaged in gaurding against one extreme, in the practice of medicine, he should be as much so in avoiding the other. Whether he who prescribes too boldly, or he who does it too timidly, does the most mischief in society, is a question not easily solved. The fearful practioner trifles with his patient and suffers the disease to become so firmly fixed as never to be cured, when one with more energy vanquishes it, in its forming state. There are, doubtless, many remedies of inestimable value, which by

being given in too small a quantity, have not the wished for effect, and are consequently abandoned as inefficacious, when had they been properly administered, they would have the the desired effect, and be justly esteemed.

I attribute the success of this case in a great measure, to the muriat of Barytes. How are we to account for its operation? Is it necessary to have recourse to the irrational doctrine of specifics? I hope not.

Scrofula is a disease of the system in general, and of the glands in particular; and is, perhaps, owing to a morbid affection of the absorbents. Should this be the fact, are we to attribute the success of the Barytes, to any particular operation on or through the absorbents? I presume not. From practice we are warranted in believing that there are many medicines that operate much more powerfully on the absorbent system than Barytes. Mercury and Digitalis both have, I believe this effect. Mercury was at one period prescribed in this disease from its supposed specific power on the diseased glands or on the absorbent system; but except in combination with other medicines, it is now, I believe generally discarded, not only as useless but pernicious in this disease. And Digitalis, although its power on the absorbent system is truly wonderful, yet it would no doubt be very pernicious in Scrofula* I therefore think it rational to suppose, that the medicines which are salutary in this disease are so from their general tonic power; and not from any specific property. It is possible, nay even probable that the muriat of Barytes and the muriat of lime, medicines at present the most celebrated in this disease, may operate on the absorbent system in an increased proportion to that of the general system. However, I believe their effect is not confined to the absorbents, but that their salutary power is

*When I wrote the above lines I did not think that the Digitalis had ever been used in this disease; but on looking over Beddoes on Scrofula I found the following sentence. "One day only I ordered a few drops of the Tincture of Digitalis, but on the occurrence of some unfavourable appearances, probably not owing to the Digitalis, I relinquished that medicine."

first communicated to the stomach, adding tone to its muscles, enabling it to perform more completely its function, thence to the chylopoetic system, and so on until the general system is completely renovated.

This conclusion is rendered at least rational, from the experienced utility of the corroborating medicines that are generally used with them; and also from the plan of practice pursued by those who have studied the disease.

The cold bath is certainly a very powerful remedy in many diseases of an atonic nature, and in none more so perhaps then the one in question. Its effects are general and not confined to any particular system.*

External applications, are, I conceive, of material service in this disease. The most active stimulants that can be employed without producing ulceration or corroding the skin, I suppose the most salutary. They stimulate the glands to an increased action and give additional energy to their absorbents. I also believe that washing the parts frequently may be of service. My object was not only to keep the parts clean, that the exhalants might perform their proper function, but also to keep up, for some time, a friction on the diseased glands, whilst washing with the bitter herbs, &c. which would also act as a local stimulus.

It is now (1808) near two years since I attended this patient, and there have been no returning symptoms of disease. There is a circumstance attending this disease, which, I conceive, should teach us a truth of the greatest magnitude, i. e. that no disease should be considered incurable. Although we may not have yet arrived at that highly to be desired point, yet, I believe it attainable, and that the day may come when the most formidable disease of whatever name may be cur-

* Those who have read my essay on cold, published in the Medical Museum of Philadelphia, conducted by I. R. Coxe, M. D. will be easily enabled to apply my practice, in the present instance to the principles there inculcated; and for those who have not, I will here merely observe, that I consider cold a sedative, and believe the cold bath to have a beneficial effect in Scrofulous cases from its accumulating the excitability and hence increasing the effect of subsequent stimulants.

ed. Although I contemplate with pleasure an improvement of this nature in the practice of medicine ; yet, I believe that day never will come when diseases of any name will not terminate fatally, even in the hands of men the most profoundly skilled in the healing art.

It may be thought by some, that I have been too prolix in the minutiæ of the treatment of this disease. I am, however, confident that our frequent want of success in the treatment of obstinate diseases, is either owing, to our not paying sufficient attention to the minutiæ, or to a want of perseverance.

The following case which will conclude this part of my subject may be introduced in this communication, with more propriety than any subsequent one.

In 1807, I attended a patient, aged eight years, who was alarmingly affected with the Scrofula.

The general sy stem was much emaciated. Seat of disease, the cervical glands &c. also, the parotids, with all the glands in these parts. Some of the diseased glands were as large as a pigeon's egg. The cluster of diseased glands, on each side of the neck, extending from the submaxillary and parotids, down to the cervical, looked not much unlike two thick bunches of grapes.

Nearly the same treatment was pursued in this case as in the preceding, except not being able for some time to procure the Barytes, the muriat of lime was used. I was afterward fortunate enough to procure the Barytes. From my experience in this case, as well as others, I am positive in the belief, that the Barytes is by far the most valuable medicine in Scrofula.

In this case our prescriptions were very imperfectly attended to, but they had nevertheless the wished for effect.

I have had other cases of Scrofula, where the disease has been slight, cured by the common tonics.

With respect,

I am yours, sincerely,

G. WILLIAMSON.

Nathaniel Potter, M. D

From the Philosophical Transactions.

ON THE STRUCTURE AND USES OF THE SPLEEN.

BY EVERARD HOME, ESQ. F. R. S.

MR. HOME while investigating the structure of the stomachs of different animals, observed, that in the fourth stomach of ruminating animals, in the single stomach of others, and even in the human stomach, there is a division, more or less evident, between the cardiac and pyloric portions; that while digestion is going on, these portions are seperated by means of a permanent or muscular contraction; that the food is prepared, softened, and dissolved in the cardiac portion and the chyle separated in the pyloric.

In some animals, as in the water-rat the separation between the cardiac and pyloric cavities is very remarkable, and their difference of structure and function abundantly evident; but, in all stomachs, this difference can be traced.

"The first instance in which this muscular contraction was observed in the human stomach was in a woman who died in consequence of being burnt. She had been unable to take much nournishment for several days previous to her death. The stomach was found empty, and was taken out of the body at a very early period after death. It was carefully inverted to expose its internal surface, and gently distended with air. The contraction was so permanent, that after the stomach had been kept in water for several days, in an inverted state, and at different times distended with air, the appearance was not altogether destroyed. Since that time, I have taken every opportunity of examining the human stomach recently after death, and find, that this contraction, in a greater or less degree, is very generally met with. That the food is dissolved in the cardiac portion of the human stomach, is proved by that part only being found digested after death: the instances of which are sufficiently numerous to require no addition being made to them.

"This could not take place, unless the solvent liquor was deposited there. Mr. Hunter goes so far as to say, in his paper on this subject, there are few dead bodies in which the stomach, at its great end, is not, in some degree, digested.—That the chyle is not formed there, and also, that it is completely formed before the food passes through the pylorus is proved by the result of some experiments of Mr. Hunters, made upon dogs, in the year 1760; and as they were instituted for a very different purpose, that of determining whether the gastric juice is acid or alk aline, the results were detailed without any possible bias.

"In all the dogs, the food was least dissolved, or even mixed toward the great end of the stomach, but became more and more so towards the pylorus; and just within the pylorus, it was mixed with a whitish fluid like cream which was also found in the duodenum.

"From the result of these experiments," continues Mr. Home, "as well as from the analogy of other animals, it is reasonable to believe, that the glands situated at the termination of the cuticular lining of the vœsophagus, which have been described, secrete the solvent liquor, which is occasionally poured on the food, so as to be intimately mixed with it before it is removed from the cardiac portion; and the muscular contraction retains it there till that takes place. Such contraction being occasionally required in the stomach, accounts for its being more or less bent upon itself, which renders it more readily divided into two portions, by the action of the muscular fibres at that part where the angle is formed."

We have thought it necessary to make these extracts from Mr. Home's papers on the scructure of the stomachs of different animals, in order that those of our readers who have not yet had an opportunity of consulting them, might the better understand the account we are to give of his observations on the structure and uses of the spleen. And for the like reason, we shall subjoin Mr. Home's general conclusion, drawn from the series of facts and observations detailed in those papers.

" The solvent liquor is secreted in glands of a somewhat similar structure in all animals, but much larger and more conspicuous in some than in others."—" These glands are always situated near the orifice of the cavity whose contents are exposed to their secretion. The viscid substance found on the intereal membrane of all the stomachs that were examined recently after death, is reduced to that state by a secretion from the whole surface of the stomach which coagulates albumen."

This appears to be proved by every part of the fourth cavity of the calf's stomach having the property of coagulating milk.

" This property, in the general secretion of the stomach, leads to an opinion, that the coagulation of fluid substance is necessary for their being acted on by the solvent liquors and a practical observation of the late Mr. Hunter, that weak stomachs can only digest solid food, is in confirmation of it.

" In converting animal and vegetable substances into chyle, the food is first intimately mixed with the general secretion of the stomach, and, after it has been acted on by them, the solvent liquor is poured upon it, by which the nutritious part is dissolved. This solution is afterwards conveyed into the pyloric portion where it is mixed with the secretions peculiar to that cavity, and converted into chyle." [See philosoph. transac. part 2d. 1807.

Believing, then, that the cardiac and pyloric portions of the stomach are separated from each other by a permanent or muscular contraction, and that they perform different and distinct functions during digestion. Mr. Home, in the paper now before us, endeavours to shew, that there is between the cardiac portion of the stomach, and the circulation of the blood, a communication through the medium of the spleen; and that the use of this organ is to carry off the fluids from the cardiac portion.

1st. He observes that the fluids are chiefly found in the cardiac portion, while the consistence of the pyloric are com-

monly of uniform constance. It seemed, therefore, probable, that the fluids beyond what are necessary for digestion are carried out of the stomach without ever reaching so far as the pylorus.

2dly. He made the following experiment. The pylorus of a dog was firmly secured by ligature, and five ounces of water, coloured with indigo, were injected into the stomach; at the end of half an hour, two ounces of nearly colourless fluid were rejected by vomiting. The dog was now killed. Though the pylorus was completely closed by the ligature, the pyloric portion of the stomach was empty and contracted. "The cardiac portion contained about two ounces of solid contents, enveloped in a gelatinous substance, and one ounce of water with little or no colour, the indigo being completely separated from it, and spread over the surface of the internal membrane." Two ounces of fluid had therefore escaped from the stomach in half an hour. No absorbents could be seen; but "the spleen was turgid, unusually large and its external surface very irregular. When cut into, small cells were every where met with, containing a watery fluid, and occupying a considerable portion of its surface."

These cells he finds to be the glands of Malpighi,and cort-puscles of Cuvier.

These cells, are only distinct, and seen to contain a fluid, when examined immediately after the stomach had received an unusual quantity of liquids.

The fluid escapes when the cells are punctured. The cells are membranous, and have numerous arterial branches and plexus of veins ramifying on their coats.

The trunk of the splenic vein compared with that of the artery, Mr. Home found to be in point of size as five to one.

Mr. Home repeated his first experiments, making use of an infusion of madder instead of a solution of indigo, in hopes that the colouring matter might be detected in the spleen. The result, however, did not correspond to his expectation.

At last, on the suggestion of Mr. Brande, he made use of rhubarb, which speedily passes into the blood, and tinges the urine very soon after being received into the stomach, and very small portions of which can be detected by means of the caustic alkali, used as a test.

"Five drops of tincture of rhubarb, added to three ounces of water, are found to strike an orange tint, when the test is added. By means of this test, the rhubarb was detected in the urine of different persons, in seventeen and twenty minutes after the tincture had been swallowed. These circumstances being ascertained, Mr. Home performed the following experiment.

"On November 17th, 1807, at 35 minutes past 11 o'clock, five drachms of a mixture of tincture of rhubarb and water, in the proportion of a drachm to an ounce, were injected into the stomach of a dog, whose pylorus was secured.—At 20 minutes past one, two ounces of fluid were brought up by vomiting; ten minutes afterwards, another ounce of the mixture was injected, as were nine drachms more at half past 4 o'clock.

"The two last portions were retained, and at 8 o'clock in the evening, the dog was killed.

"On examining the parts after death, the pylorus was found to be completely secured; the stomach contained about two ounces of fluid; none of the absorbent vessels passing from its great curvature were in a distended state, so as to be rendered visible. The spleen was turgid as in the former experiment, and the urinary bladder full of urine.

"The urine tested by the alkali, received a deeper tinge of rhubarb than the human urine, after rhubard had been taken three hours by the mouth, and in other respects resembled it.

"When the spleen was cut into, the cells were particularly large and distinct. A portion of it then was macerated in two drachms of water for ten minutes, in a glass vial. All the parts were exposed to the water, by its being divided in all directions. The water thus impregnated was strained

off and tested by the alkali, and immediately the reddish brown colour was produced in the centre and no where else, but in less than a minute it began to diffuse itself, and extend over the whole. A similar portion of the liver was treated in the same way, and the alkali was added to the strained liquor, but no change took place whatever. In this experiment the rhubarb was detected in the juices of the spleen, as well as in the urine; and as there was no appearance of it in the liver, it could not have arrived there through the medium of the common absorbents, carrying it into the thoracic duct and afterwards into the circulation of the blood."

Mr. Home here concludes, observing, "the discovery of this fact I consider to be of sufficient importance to be announced to the society, that when it is thus made public, I may be at liberty more openly, and on a more extensive scale of experiment, to prosecute the inquiry."

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Dallam, on the use of Cotton, in Burns.

DEAR SIR,

I RECEIVED your favour of the 22d instant, and hasten to communicate to you the result of my inquiries and information respecting the accidental discovery of the beneficial effects produced by the application of *cotton* to *scalds and burns*. Should you think the facts stated worthy a place in the Lycæum, they are entirely at your disposal.

CASE I.

A child of Captain R. in this neighbourhood, aged five years, was standing alone before the kitchen fire when a large kettle of *boiling water* fell and was suddenly dashed over nearly its whole body. The affrighted mother who was siting in the adjoining room picking and carding cotton, flew to her darling child's assistance, and having undressed it as quickly as possible, discovered it to be badly scalded. No

medical aid being near; in the agitation and distress of her distracted mind she seized a large bundle of cotton and applied it over the whole of the scalded parts. Soon after the application, the tortured and screaming infant became perfectly quiet, and fell into a gentle and easy slumber. The cotton was suffered to remain on several hours, and when it was removed there was not the least appearance of inflammation remaining. This case was communicated to me by a person of veracity who saw the child soon after the accident and there can be no doubt of its correctness.

CASE II.

Soon after the termination of the preceding cure, a black child belonging to Mr. S. of this county (Harford) aged eighteen months, while playing on the hearth, fell into the fire and was badly burned. Its screams drew its mistress to the kitchen, and having just before heard of the successful experiment with the cotton, she immediately covered the parts with a thick boulster of the same. She informed me that it succeeded completely, and that she had been convinced of its efficacy, although she had before no faith in it.

CASE III.

About three months ago *my own little daughter*, was crawling unobserved near the hearth and overturned a tin cup of boiling water, which a servant had just before brought to me to make a lather for the purpose of shaving. The water (which was boiling) extended over the whole of one of her arms and scalded it exceedingly. Being present, I immediately bound up the effected parts with fine cotton. She ceased crying in a few minutes, and was well enough to ride to Havre-de-Grace with her mother the same day. When the cotton was removed the cuticle was scarcely discoloured.

In what manner does cotton act in lessening pain and preventing or removing inflammation from scalds and burns? Is it by excluding atmospheric air simply, or by some spe-

cific power? Whatever theory may be adopted to explain the facts, they must remain undoubted, and if a still greater number would add to their validity, I could state them, for I have witnessed the same effects from the same application in many other cases. I presume those I have detailed will be sufficient to encourage others to give the remedy a fair trial.

I am moreover of the opinion, that the application might be serviceable in other inflammations if properly applied.—The following case would seem to favour such an opinion.—A respectable lady of this county was seized with a violent pain in one of her jaws, which was supposed to have been rheumatic. The pain was excruciating and the inflammation and swelling great. The usual remedies had been applied for several days without affording the least relief. She was advised to try the external application of cotton to the inflamed part. It succeeded completely, and she assured me that in twelve hours after the application the inflammation and swelling intirely disappeared.

Accept, dear sir, of my best wishes for the success of the Lycæum, and believe me

Yours very sincerely,

WM. M. DALLAM.

Harford County, January 20th, 1811.

...

The simplicity of the application of cotton in obviating the action of fire upon the living fibre, and the want of success in the use of means much more plausible, left my mind in a state betwixt incredulity and approbation. Having sometimes failed in the treatment of inveterate burns by all the modes hitherto recommended, and having lately been disappointed in the effects to have been expected from the spiritus Terebenthinæ (the celebrated remedy of Doctor Kentish) where gongrene ensued notwithing its immediate application. I resolved on availing myself of the first occasion of testing the efficacy of the cotton. Only a few days had elapsed after the reception of Dr. Dallam's letter before a case in all respects adapted to my views presented itself.

On the evening of the 7th day of February, while I was visiting the lady of Mr. John Leigh of this city, the family was suddenly alarmed by the cries of William Wheatly, an adopted child of Mr. Leigh, aged thirteen months. He was immediately brought to Mrs. Leigh's room where I was sitting, and upon examination I found that boiling water had been accidentally poured down his back from the spout of a tea kettle. The water first came into contact with the cuticle between the scapulæ, opposite the second dorsal vertebra and descended along the spine and an inch laterally to the right and left till it reached the glutæi muscles. In ten minutes after the accident, a covering of finely carded cotton was laid over the whole affected surface and confined in close contact with the cuticle. As the cries of the child were expressive of the most excruciating pain, and the solicitude of the familyfor its repose great, Iwas questioned respecting the propriety of an anodyne? Having determined to rest the case solely upon the virtues of the cotton, I evaded the question and resolved to remain with the patient, and observe as minutely as possible the progress of the case. In about fifteen minutes after the application the child become less agitated, in half an hour ceased crying and a profoud sleep ensued in forty minutes from the accident. The child continued to sleep for about an hour, when he suddenly awoke, and after crying a minute or two became composed. The cotton was now removed and another boulster applied. No mark of increased action was apparent, and a tranquil sleep soon followed. The child remained in this state unmolested by pain or restlessness till about six o'clock the next morning, when it awoke, resumed its usual habits of playfulness and good humour, took its usual morning repast and discovered no symptoms of disease. Agreeably to my request the cotton remained on the scalded parts till I removed it at nine o'clock. It was with difficulty I could trace even a rubefacience although the appearanccs had been so threatning the evening before. There was however a small exception to this pleasing appearance. There was a space of about an inch below the place first touched by the water that was vesicated. As I had not observed the precise situation of the cotton before it was removed, I was at a loss to account for this limited affection; I was informed by one of the attendants that the cotton had slip-

ped down during the night, and that the vesicated part had remained bare. The application was renewed as well to the vesication as to the other parts, and continued till the morning of the ninth, when it was finally removed. The vesication gradually declined, and on the twelfth the cuticle peeled off without the least ulceration, or even a discharge." Although I cannot satifactorily account for the operation of the cotton, I will state an occurrence that may lead to an experiment to determine it hereafter. Although the heat of the affected parts was augmented previous to the application it was afterwards dimimished to a degree less than the ordinary temperature of the skin, for on the removal of the application the parts were cooled while the cotton was preternaturally heated. It was to have been expected that the preternatural heat of the skin would have been communicated to the cotton till an equilibrium would have been produced according to the usual laws of coloric; but the fact was otherwise, for, the cotton was heated in the same ratio that the skin was rendered cooler. If it were admissible to call to our aid the electric fluid so conveinent and fashionable in cases of difficulty, we might hazard an hypothesis that cotton might be an excellent conductor, and possibly obey laws essentially different from those observed by common electricity or the matter of heat. EDITOR.

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Wright on the Malignant Fever of Elkridge Landing and its vicinity.

THE prevalence of epidemic diseases now rests for elucidation upon the assumed basis of atmospherical influence. To confer upon their history its merited interest, and give to it the most extended prospect of beneficial influence, a meteorological register is a necessary appendage; or to say the least, an important auxiliary. I regret that it is not in my power to give one in the present instance, circumstances having precluded the possibility of bestowing upon it the necessary attention.

I have met with no record in the history of the bilious diseases of our country prevailing in situations remote from populous cities, which for malignity of symptoms, affords a

parallel, or will bear a competition with the epidemic which ravaged this district, for three months of the last autumn; except, perhaps, in circumscribed spots, peculiarly favoring the origin and local concentration of the prolific agent in the production of this disease. Elk Ridge Landing is situated at the head of tide water, on what is called the Ferry Branch of Petapsco. For a considerable distance above, the river winds in a narrow sheet between two extended hills of very great and nearly equal elevation, in many places so dammed up, or obstructed in its course, as to afford reservoirs for mills and other manufactories. At this place however and below it, the hills still preserving their altitude, have opened an extended space, presenting in the interval a low and level champain, now cultivated as meadows and grazing grounds.

If the presumption with regard to the active agent in the production of bilious disease be correct, it is not to be wondered that in situations like this, it should successfully exert its baneful influence. From an abundant source of miasmatic effluvia, it rises involved with the evaporatory process, and finds an equilibrium of specific gravity, below the elevation of the river's bounds, where reverberating from hill to hill, it is receiving daily addition and daily concentration. It has been remarked to me, as the result of observation, by some of the old and intelligent inhabitants of this district, that in seasons when the river was swelled by frequent freshes, the neighbourhood was less the seat of disease than under opposite circumstances. Where freshes are frequent, and repeated at short intervals, this opinion would appear founded on plausible inference, for every addition of pure water might reasonably be supposed to assist in the dilution of the miasma, or in carrying off its matrix. The late season however resulted reversely, and contradicted this conjecture. A few days antecedent to the first attacks of bilious fever, the river had been swoln by a fresh to an extent rarely occuring at a similar season. I was from home when it occurred, but a gentleman of respectability informed me, that it brought

with it a very considerable and highly disagreeable smell, which induced him to suppose, that some one of the numerous dams had given way, and that the evolution of its collected filth, produced the stench, this was about the 26th of July. On the 1st of August, at 2 o'clock, I was called to attend a lady, at the landing, between sixty and seventy years of age, and found her labouring under bilious fever. She was in the act of vomiting when I entered her room; it was spontaneous and she was throwing up bile of a pale green colour. This bile came away in lumps or clots about the size of a pullet's egg, of the consistence of jelly, but possessing considerable tenacity. I administered warm water and ordered the immediate preparation of weak chamomile tea. It was brought before the emetic effort was over, and she drank freely of it, and by the time that effort ceased, she had discharged nearly a quart of bile: the last came away fluid. I now obtained a history of the attack which came on at one o'clock, with indistinct chill, accompanied with slight shivering or horror, and unattended by any local pain. The pulse after the vomiting, was quick, soft and moderately full, the habit of the patient robust, considering her age. The symptoms were now abated and she was tolerably easy, but complained of some heat. I gave for the present spt. nitr. and ant. vin. as a sudorific, and at night she took four antibilious pills. These had not operated well next morning early, and she then took jalap and chryst. tart. twenty-five grs. each. They operated briskly and freely. I left her at twelve much relieved, but at one was hastily called to her again. She had had, as before, a slight chill, and as on the preceding day was vomiting freely when I entered the room: she was discharging clots of bile of a similar appearance with those of the day before, with one exception, they were now of a dark green colour, and she complained of pain and burning in the stomach. The same course was observed as at first, the vomiting liberally encouraged. To me it was a subject of astonishment to wit-

ness the quantity of bile thrown up, a small chamber vessel was nearly filled with it. When the vomiting ceased she seemed considerably exhausted, and a little warm wine and water was given her. The day was extremely sultry, and the exertion of vomiting under febrile action had produced free perspiration; it was highly offensive. The house had been thrown open for air, and this smell diffused itself into the adjoining apartments, two or three persons indeed who came to the porch where I was sitting to enquire how she was, perceived it and asked from whence it proceeded. Her face had now assumed, and preserved even in fever a sallow-like complexion, but there was no apparent bilious effusion on the tunica abbuginea; she complained of fullness of the stomach. Without waiting for a perfect apyrexy, I prepared and administered a large dose of calomel and jalap; it operated well and had ceased at midnight; she slept afterwards and was easy next morning, skin cool, pulse slow and soft, her tongue had not been furred from the first, it was now white and moist. She began this morning small doses of bark and wine, gradually increasing as the hour of expected accession came on; at eleven o'clock five blisters were applied, to the ankles, wrists and one over the stomach; by twelve these were in action, half an hour after æther was combined with the bark and wine, and repeated in fifteen minutes. At one, without any previous indication, she was seized with chill instantly succeeded by cholera. The dark bile poured copiously from the stomach and bowels, it resembled coffee grounds and towards the last was mixed with blood. Nothing would check it and she died at three.

On the evening of the same day upon which this lady died, I was called to a man about a mile distant; when I entered the house, I was assailed by a smell which drove me instantly from it, and strongly impressed my mind with apprehensive forebodings. The smell proceeded from an evacuation; I called the family and requested that some sugar and vinegar might be burnt in the room. His wife

then informed me that he had been attacked two days before with chill, &c. and on the preceding day had taken a purge which operated well, that he had not been confined except during the paroxysm, and on the morning of the present day had eaten a hearty breakfast, and walked about until two o'clock, when he was attacked. I entered the house and found him dying; he expired in half an hour. When I re-returned to the landing I was requested to visit a young man sixteen years of age, who had been attacked the evening before with chill and vomiting. The discharge resembled coffee grounds, delirium had supervened at midnight and still continued, it was of the low comatose kind. The pulse was quick and small, the countenance Hippocratic, and the complexion deep orange, of which the trunk and extremities equally partook. When much effort was made to rouse him it succeeded partially, he obstinately refused to take medicine, and a fixed spasm of the baccinator and orbicularis oris prevented its forcible introduction. His parents objected to any farther effort and he was trusted to nature; on the second day in the evening a profuse general perspiration broke out, it soaked through the bed and dripped upon the floor. He recovered rapidly.

From this period until the first of November, this hydra-headed monster brandished the besom of destruction over the whole neighbourhood; man, woman and child were equally the subjects of its universal ravage. Anxiety for the ill, grief for the dead, or the forcible expression of corporeal suffering and mental alarm, were legibly written upon every face, and hung upon every countenance a perpetual gloom.

The necessary limits of such a communication as the present will not admit a comprehensive view of its subject. I will give its form of attack, course and termination, with what appeared to me the most successful mode of treatment.

As to its character it had none, and the late season is the first, in which I ever witnessed the indiscriminate prevalence

of the remitting and intermitting form. Hitherto as far as my observation of bilious fever extends, when it assumed in the commencement the remitting character, it has preserved it through the season; intermittents were rare, and vice versa. Of the comparative severity of their character also, I had formed an opinion founded upon specious principles, which the late season materially affected, viz. that the twenty-four hours calm afforded for repair and preparation, enabled us to guard against or ward off the expected storm; but this calm was deceitful and those efforts unavailing in the late squalls.

With the general pathology or distinguishing characteristics of bilious fever, we are all acquainted, I shall pass them in silence, and pourtray only the prominent features of the one whose history I am recording. In the early part of the season its attacks were generally of the remitting kind, towards the close of it, they assumed the opposite character. The chill which marked the first inroad of this disease was extremely indistinct, disclosed only by faint shivering, livor of the countenance, cold extremities and the sensation of a cool aura passing up and down the back. This was so much the case that the ignorant part of my patients very frequently contended that no chill had preceded the attack. There were however some exceptions, and in those cases the extremities were very cold, and felt like the appendage of a foreign body, the lips and nails were purple, the countenance livid or verging to black, the eyes dull and heavy, the respiration laborious accompanied with singultus, and a sensation of universal muscular rigidity, or as it was termed numbness. The stomach was generally pointed out as the principal seat of disease, not often a nausea, but more commonly a sensation of fulness, heat and burning attended; it was called by the old people a colic; the head sympathised with it, by pain generally of the frontal section, with a prominent, heavy, watery eye. The pulse in the chill was contracted and scarcely perceptible, in the reaction it was not full and round, but soft

and frequent. Perspiration was partial and fetid, confined chiefly to the head and face, the decline of the fit was by slow and gradual exhaustion. The tongue in the first paroxysm, was white at the edges with a streak of discolored mucus in the middle, moist and not furred; in the second, still moist, covered with dirty slime resembling soft soap, the glands enlarged, and furred; if they lived through the third, covered with a dry brown crust, contracted and inflamed at the edges rendering articulation difficult. I have said the head sympathised with the stomach, this was to a very great degree; it came on generally in the second or third paroxysm, producing obstinate coma, low delirium, and not unfrequently apoplectic symptoms, it uniformly proved one of the most dangerous attendants, I saw but one case of active mania. There was at first no pain in the region of the liver, but it not unfrequently became sore to the touch as the disease proceeded. In a great majority of the cases which came under my notice, the system generally was in a very low state of torpor, the intestines particularly could be urged into action only by the most powerful medicines. Where evacuants acted freely substituting a sensation of emptiness and ease for the before existing fulness and burning of the stomach, where the tongue became clean and the system rose into free and general action under their operation, the patients recovered rapidly and perfectly. On the contrary, where the action of medicine was partial, or indeed where this was not the case but they acted well, if the foulness of the tongue, the fulness and heat of the stomach, the irregular action and general torpor of the system remained or increased, the case was doubtful and the termination generally fatal. Where this was the result the patients generally went off in the quiescence of chill, or sometimes lingered a little longer in the process of mortification. I saw no case of gradual exhaustion from supervening typhus. In either event of recovery or death, they were restored speedily or their sufferings soon terminated.

TREATMENT.

EMETICS.—In the generality of cases in which spontaneous vomiting came on early, it proved a favorable symptom. It was a nisus naturæ which pointed out the demand of the system; it was also an argument of the stomach's sensibility. To encourage the discharge I was at first in the habit of using warm water and chamomile tea, afterwards with better effect large draughts of a weak watery solution of the antimoniated tartarite of potash. Frequently however I found it necessary to give the emetic draught in full and free doses; the event was always satisfactory. I treated no case in which I regretted the use of this highly beneficial preface to the evacuant plan in bilious fever. Although the natural enixus alone has not been frequently effectual, it nevertheless appears to point out the proper mode, and instead of increasing that debility so formidable to some, nature relieved by evacuation from a state of oppression, will quickly rise into more healthy and vigorous action. I have somewhere seen an opinion advanced that bile becomes attached to the villous coat of the stomach and intestines like dirt to the skin; the difficulty of dislodging it, the solution of the disease by repeated and active evacuants only, and the very considerable quantity of inspissated tenacious bile always following their effectual exhibition, seem in some measure to countenance such an opinion. However this may be, I found the shock given to the system in vomiting essentially serviceable in rousing it from its general and dangerous torpor, in pouring out by compression and agitation the retained bile of the choledochus, and in relieving the stomach of its load. In one case I gave an active emetic to a man apparently dying, and with success. There may be however, and no doubt are certain states in bilious fever which forbid the exhibition of emetics; such as particular local affections of the breast and head, and absolute prostration from extreme exhaustion: but these are rare. I followed the emetics immediately by,

CATHARTICS.—These were given as soon as the stomach was sufficiently quiet to retain them. For some time I used calomel and jalap in common doses, proportioned as near as possible to the habit of the patient; this did not succeed agreeable to my wishes; it was too slow and partial in its operation, and my patients were not generally relieved by it; for this I could afterwards account, I did not give it in the doses which I eventually found necessary; and which in truth I had no idea the system would bear. The next form of purge I used was jalap, crem. tart. and tart. emet.; this last was so combined as to sicken. With the operation of this I was much pleased, it seemed by nauseating to open the ducts and free the liver, and by relaxing the system to facilitate the transit from the bowels. It also produced a determination to the surface. This however I laid aside as not sufficiently powerful and too bulky, and returned once more to the calomel and jalap. I now gave sixteen and twenty. On the tenth of September I was called to a young lady twenty years of age, of a good constitution but bilious habit; I found her vomiting a fluid of the colour of a strong solution of indigo. The emetic draught tolerably strong was given and operated actively. After the vomiting had ceased she complained of pain in the head, breast and stomach; her pulse was full and springy. I took away twelve ounces of blood; this was the first blood I had shed in this fever. The pulse was reduced, and the head and breast relieved, but the burning at the stomach continued. She took a solution of carbonate of potash, and soon after sixteen grs. calomel and twenty of jalap in six pills. They shewed no disposition to operate by next morning, she then took thirty and thirty, and to support the operation I prepared six pills calomel each six grs. and left them, with orders that one should be taken every three hours until a free operation should be produced. She took them all in twenty-four hours. At night her pulse had risen under the operation, and I took away more blood. A solution of phosphate of soda was given in small doses through the night; she was much better next day, and soon recovered. The same patient relapsed three

times in the course of the fall, was highly bilious each time, was treated always pretty nearly in the same way, and with success. The calomel each time affected her mouth; she took in the aggregate 180 grs. This plan of purging I afterwards pursued with the most pleasing result: except when circumstances absolutely forbid it. Thirty or forty were my usual dose, this was worked off with calomel pills, which frequently affected the mouth, and always with benefit. A respectable physician in the neighbourhood informed me that his medicines succeeded best where his patients fainted under their operation. When purgatives were commenced, I found it always best to continue or keep up their action by the introduction of others upon the decline of the preceding ones; four, five, six or more, were sometimes necessary in proportion to the ease or difficulty of dislodging the bile, the powers of the patient and the relief afforded the system. In forming an estimate on this head, the tongue was always a useful and I believe invariably correct oracle; I mean by its appearance. It uniformly became clean and assumed a healthy look, when the stomach and bowels were sufficiently unloaded, and not till then. The evacuant plan however was necessarily moderated in particular cases, and governed by existing circumstances. Some of my patients required a more delicate treatment; I will insert an example. On the eighth of October, I was called to a woman about thirty, of delicate habit. She had passed through two paroxysms of chill and fever of the remitting kind; she had been bled and taken a purge which operated well, but produced no relief. When I saw her about eight o'clock, A. M. she appeared extremely languid, with weak frequent pulse. Her stomach was very irritable, she was throwing up at intervals of only a few minutes the coffee-coloured fluid, and complained of burning or as she termed it knawing at the stomach; her respiration was anxious, accompanied with singultus. The period of attack on the two preceding days had been twelve o'clock, and another paroxysm I apprehended would prove fatal. She took a solution of carbonat. potash with essen. menth. pip.

and laudanum, to quiet the stomach; at eleven, P. M. hot cataplasms of mustard were applied to the feet so as to cover them entirely, and the same application was made to the stomach, arms and hands. The compound saline mixture checked the vomiting, and the chill did not come on. At two o'clock, the cataplasms were removed and the mixture discontinued. She now commenced magnesia in large doses, and continued them through the night; they operated smartly, and every operation diminished the burning, and took off the determination to the lungs. The stools resembled molasses or tar, and were very fetid. On the next morning, the magnesia was discontinued and very strong chamomile tea given; the sinapisms were again applied at eleven; she again escaped chill and returned at two to the magnesia. By pursuing this course for one more day, she was freed from danger. Sulph. sod. and tartarized antimony combined, composed another form of carthartic which I used in many cases of this fever. I used them generally in large doses for three or four mornings successively; where I wished to evacuate rapidly, they were particularly useful appearing to cool while they emptied the system.

BLEEDING.—It was not until about the first of October that I met with many cases which required, or would safely admit the use of the lancet. They had now assumed more of the intermitting fever, and were more generally accompanied with active febrile exacertation. I used it however in every instance when the powers of the system were not too sensibly depressed, in which cases I found emetics most useful in rousing those powers into action by unloading the stomach. In the first stages of the disease of this month, it was very frequently admissible, and in moderate quantity I found it a powerful remedy against all the symptoms. It is the neatest, speediest, and certainly a very safe method of moderating action, as we have it so completely under our controul. It takes off that troublesome determination to the head, and used in the exacerbation more certainly cuts short the paroxysm than any other remedy. The observation of

writers who class bilious fevers among pyrexial asheniæ, that the simple intermittent is by the depleting plan of cure, frequently brought into the continued form of typhus nervosa, I apprehend to be incorrect. Injudicious depletion may sometimes be a cause, but for the most part I am inclined to believe that a neglect of the depleting plan in the commencement lays the foundation for the typhoid character, and that the irregular, forcible and uncontrolled action of the vessels is what shatters the system and protracts its powers. I am aware that with many respectable authors the remote causes of this disease are certain sedative powers applied to the system inducing or increasing a state of debility is held the proximate cause. The predisposing causes are esteemed a state of constitutional weakness, previous disease, evacuations of every kind, &c. &c. This opinion has been much advocated, and seems to have been taken up more upon the ipse dixit of writers, than from observation. The varieties of situation have been left out of the picture, and it seems to have been forgotten, that most of those opinions have been given to the world, by writers of some of the warmest climates of Europe. One of those writers, Dr. Dickenson, of the Edinburgh Medical College, who has taken great pains to collect into one view, the phenomena, causes remote and proximate, and modus medendi in fevers, has gone so far as to observe that evacuations by purging in intermittent and similar fevers were extremely injurious, that bleeding was certain destruction, and that even a dose of nitre had in some instances almost irretrievably sunk the patient. In the course of his enquiries he goes on to blame the timidity of those who had hesitated to administer wine, bark and opium in the hotest stages of intermittent paroxysms. I can only observe, if this be correct practice the bilious diseases he is describing must be essentially variant from those of our own country. It was not always under those circumstances alone, where I was sure I was safe in bleeding, that I ventured to perform the operation. I met with several cases where the decision was delicate, yet I hazarded the practice. These

were cases in which I did not see the patient until two or three days had elapsed, and the disease had ravaged unopposed: here there was much depression of the system accompanied with burning and pain of the stomach. In the first case where the experiment was tried, a large dose of calomel and jalap had been given, followed by ol. ricini and injections which produced no operation. On an attentive examination, the tongue was dry and inflamed, the serous vessels of the eye contained red blood, and the bilious hue of the surface was interspersed here and there with inflammatory streaks; the respiration was laborious and the head pained. Bleeding unlocked the system, medicine acted well after, and the patient recovered. In similar cases afterwards I did not hesitate, and I had much reason to be pleased with the practice. The pulse in these cases was small, but rose upon bleeding. I have said in the early part of this history, that coma and low delirium were frequent and dangerous attendants upon those fevers. A physician who attended this district with me informed me that every case he had met with proved fatal. About the tenth of October, I was called to a young man of spare habit, who had been attacked with bilious fever the day before. He was lying on a bed with his clothes on, having been walking about until ten, A. M. when he was again attacked with chill. As the fever formed, delirium came on. He was now at two, highly comatose with indistinct muttering, and no efforts would rouse him in the smallest degree. The skin was hot and dry, the pulse beating one hundred and fifty and small. A large tub of warm water was prepared into which he was placed, and a blanket wrapped round him so as to confine the vapour. I then drew from the arm slowly, three pints of blood. While bleeding, partial perspiration would frequently break out, but cease again in a short time; no alteration was produced. I staid with him three hours, during which he was kept in the water, to which additions of warmer water were made as fast as it cooled. The symptoms continued the same, and before I left him I again opened the vein and took away a pint more

of blood. There were still no prospects of returning reason, and I left him with directions, that he should be kept in the bath three hours longer if necessary, and if not then relieved to be taken out, wiped dry, put to bed and hot mustard cataplasms applied immediately to his feet. I returned next morning and found him sitting up in bed. He had recovered his faculties before the period of his continuance in the bath expired. He soon got well, but his intellects were partially disordered for three or four days.

BARK AND OTHER TONICS.—Of the bark I made little use, if given before proper evacuations had been procured it aggravated the symptoms and increased the disease, and when those evacuations had taken place it was superfluous; for the system rose rapidly into healthy action. In most instances I believe the patient was dead or out of danger within ten days from the attack, the fatal remitting cases from forty-eight hours to five days, the intermittent protracted a day or two longer. In some few exceptions to this remark, a quotidian or tertian remained after the bile appeared evacuated. In those the bark was of no service, confining the patient to bed and procuring free perspiration by active sudorifics previous to, and supporting it at the time of expected accession, was the most successful in breaking the habit or chain of periodical chill or ague. This plan succeeded with me when bark, musk, blisters, camphor, opium, and æther were entirely useless. The best form of sudorific which I used was a table-spoonful of good emetic wine in a pint of Madeira: a wine glass full of it given warm every one or two hours so as to meet the fit. I used also in some instances Dover's Powder, and occasionally nauseating doses of emetic tartar and ant. powder. In some other instances where, in getting clear of the bile, an alarming state of debility was induced and chill apprehended, blisters to the wrists and sinapisms to the feet so as to be in action at the time of expected periodical accession, averted the danger most effectually. In addition to these, good porter was of service if the stomach would bear it; and after the apprehended pe-

riod was over, chamomile and columbo were the best corroborants. How far does the manifest superiority of external stimulants over internal toncis in keeping of chill, favor Cullen's theory of cuticular spasm in its formation?

MISCELLANEOUS REMARKS.

Never before, I suppose, did a disease of this kind so generally pervade a neighbourhood or district, as did the late epidemic here. I remember but two or three persons within the supposed range of this airy demon, who entirely escaped. Whether it was in any instance communicated by contagion, I should hesitate to say; there were a few cases which appeared to favor such an opinion; but as a certain degree of predisposition from improper quantity or quality of bile seems necessary to the constitution of the disease, it must remain a subject of doubtful speculation. One occurrence, which however proves nothing on this head, very generally took place; whenever this visitor entered a family large or small, it scarcely ever left it until every member of every description, had been subjected to the ordeal; and in families composed of fourteen or fifteen persons, I had frequently at the same time twelve or thirteen patients. On entering houses where this disease was raging, I was frequently attacked almost immediately with head-ache, nausea, and increased frequency of pulse. A removal into the open air, and gentle exercise on horseback would carry it off. One case which occurred to me this fall I will give, and leave without comment with regard to probable contagion. I had called to visit a woman whom I had attended some short time before in childbed; she had that morning, for the first time since delivery, left her room, and was sitting in good spirits in an outer apartment where I was conversing with her. A labouring man of the family at this moment entered in a paroxysm of bilious fever and laid down upon a chest; he was in a free perspiration and it was highly offensive. The woman complained very soon of giddiness and nausea and retired to her room, here vomiting soon en-

sued. The man died that night, and the woman a few days after. Sex, age, habit, constitution or peculiarity and incidental variety of situation, produced but little modification of this disease. The robust were more violently attacked, the symptoms more severe, the course less protracted, and the termination more frequently and more rapidly fatal, than in mediate situations. New comers into the neighbourhood, were greater sufferers than the long settled inhabitants. Blacks died less frequently in proportion than the whites; many of them however were roughly handled, and I observed in them a remarkable prevalence of languid or suffocated excitement. In the generality of those cases, the pulse was even in fever, very small and feeble, and I met with four or five in which, from the very first attack, there was no pulse perceptible at the wrists; they were dull and listless, much disposed to sleep or rather stupor, unwilling to move or speak, slightly delirious, and complained of no pain. Emetics active and repeated, and followed by brisk cathartics relieved them. In the course of the season, I saw but few cases of jaundice or the complete orange tinge: the complexion more generally resembled dirty tallow, and the capillary vessels seemed filled with a cream-coloured fluid. The tunica albuginea was frequently not at all discoloured. Petechiæ were common, and appeared generally in the decline of the second paroxysm. I met with one case where the powers of the system had weathered the storm without the aid of medicine. Here nature had produced a crisis by those local dispositions denominated carbuncles; there were twenty-four of them, containing a fluid resembling extravasated blood, or tar. The attack of this disease was in a great many instances preceded for a day or two by a constant and considerable burning in the palms of the hands. I had about the middle of September, this sensation myself, accompanied with acute pain of the head. I took away at once two pints of blood, and immediately after swallowed a cathartic; in the act of swallowing this dose, some of it passed into the trachea, and I was instantly seized with a convulsive cough; inspiration was

prevented by the violent effort to throw it off, and I was very near perishing under the suspension. In this convulsive action I felt a sudden lancinating pain, with sensation of cracking or tearing in the liver, and, in fifteen minutes, operation commenced. This was at ten on Saturday, the operation continued till twelve on Sunday, and the quantity of bile discharged was truly astonishing; it came away like thick oil, of a dark green colour, and after a few evacuations totally unmixed with fæces. By living low, seldom eating dinner and then chiefly vegetables, abstaining from liquid stimulus, except occasionally a glass of wine, and avoiding other causes of increased excitement, I escaped for the rest of the season.

The commencement of this epidemic was with the month of August, it was most prevalent and most severe about the middle of September, and declined with October. Relapses were frequent, but generally mild.

T. H. WRIGHT.

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Hopkins's Case of a Shot in the Trachea.

Anne-Arundel County, February 4th, 1811.

SIR,

I HEREWITH transmit the history of a case occasioned by the introduction of a *shot* into the *trachea*, which from its singularity and the extraordinary mode adopted for its relief appears to me worthy of record.

Perhaps there are no cases that occasion more distress and anxiety than those in which extraneous substances are intercepted in the pharynx or larynx, and none, therefore, for which we should be more studious to devise prompt methods of relief. The following are the particulars of the case to which I allude.

"A young lady of beauty and sprightliness espying a middle sized shot upon the floor, took it up, put it into her mouth and was gently chewing it while at the tea-table with several other young ladies. While one of them was relat-

ing a laughable circumstance that excited a general and sudden burst of laughter, the shot passed the epiglottes and lodged in the trachea. Violent coughing immediately succeeded to the accident and harassed the patient almost incessantly. Four physicians of the greatest medical repute were immediately sent for, who attended her for several weeks, during which time they made use of a variety of means to relieve the complaint or mitigate its symptoms; such as repeated bleedings, oleaginous demuleent and anodyne medicine, which in some measure appeased the cough, but afforded no prospect of effectual relief. In this state of the case the patient was abandoned by the gentlemen of the faculty: her deplorable situation, however, excited to an unusual degree, the sensibility and ingenuity of her mother, who, for good sense and ingenious device was scarcely equalled by any woman in the county. She put her invention on the rack to devise some scheme of relief more effectual than those adopted by the physicians. She recollected to have heard that the anatomy of a hog very nearly resembled that of the human species, and this determined her to have a hog killed and brought to her. She began a dissection and proceeded slowly from the top of the larynx down to the lungs, attentively observing the nature and use of the epilottis and thence conceived it practicable to propose some scheme to enable the shot to find its way out of the trachea. Here it is necessary to remark, that during the violent fits of coughing, the patient would often remark, that she felt the shot high up in her throat, as she expressed it. These circumstances having been revolved in the mind of the mother, she come to the resolution, that she would continue at the back of her daughter, and wait for a violent fit of coughing, and throw her with force out of bed upon her hands, supposing it probable that the fright and force added to the weight of the sh t, might eject it from the glottis. The success of the experiment justified her conjecture, for during a violent fit of coughing, the mother (without having imparted the scheme to the daughter) suddenly threw her out, as she was

bending over the side of the bed. She fell upon her hands, when, to her inexpressible satisfaction, she heard the shot roll upon the floor. From this time the patient gradually recovered, and enjoyed a tolerable good state of health for many years. She afterwards married, and had several children, but was subject to repeated attacks of hœmoptœ, and ultimately died of phthisis, palmonalis.

It is probable that the shock given to the lungs, by the long continued irritation of the shot, perhaps, increased by the poisonous quality of the lead, laid the predisposition to the disease; and the roughness of the shot, from having been previously chewed, probably added to its irritating effects.

Although the result of the above case, does not redound much to the honour of the scientific gentlemen who attended this case, some useful lesssons may be drawn from it, and therefore it becomes necessary to record it. It may perhaps, tend to rouse physicians to a more vigorous exertion, and excite them to indefatigable industry in inventing ingenious modes of cure, whereby they may supersede the necessity of quacks and ignorant pretenders, stepping in during our moments of listlessness and inattention; for by our aversion to mental exertion we often give them occasion to triumph over us to our shame and confusion. For these reasons it behoves us in this, our enlightened day, by assiduity and attention, to exhaust invention itself, for the utmost attainment of all the art it is capable of, that we may retain our excellent art among ourselves. When this shall be the case, we may predict without the spirit of prophesy, that the good sense of the community will put down quackery, and discountenace the use of nostrums, and the sale of Patent Medicines.

With the best wishes,

I am yours respectfully,

RICHARD HOPKINS.

Editor of the Baltimore Medical Lycæum.

From the Edinburgh Medical and Surgical Journal.

Report on a Memoir of Drs. Gall and Spurzheim, relative to the Anatomy of the Brain. By M. M. Tenon, Portal, Sabatier, Pinel, and Cuvier, presented to, and adopted by the Class of Mathematical and Physical Sciences of the National Institute.

THE physical class appointed M. M. Tenon, Potal, Sabatier, Pinel and Cuvier, to report upon a memoir, entitled, Inquiries into the nervous system in general, and the brain in particular, by Drs. Gall and Spurzheim.

Your committee must confess, that they had at first, some doubts about undertaking its examination.

The class has always adhered to a very wise rule of giving no opinion upon works already submitted in print to the great tribunal of the public ; it might be supposed, that the anatomical doctrine of Dr. Gall has obtained, by means of the lectures which he has delivered in the principal cities of Europe, and by means of the numerous extracts from them, published by his pupils, a degree of publicity almost equivalent to that of authentic publication.

Besides, this anatomical exposition of the nervous system is generally supposed to be intimately connected with, and, in fact, is connected to a certain degree, by its author, with a physiological doctrine which he teaches, concerning the special functions of the different parts of the cerebral organ, a doctrine which in no way comes under the cognizance of the class, since it ultimately depends upon observations relative to the moral and intellectual disposition of individuals, which certainly are not within the sphere of any academy of sciences.

Such were the motives which at first deterred us ; but others soon occurred which counterbalanced them.

Of all that has been written from the lectures of Dr. Gall, his opinions upon the anatomy of the brain have been announced with most confidence, and, at the same time, ex-

plained, with least detail and perspicuity. Besides, he does not acknowledge entirely any of the publications of his scholars; and consequently none of them enables the public to judge of his ideas, or supersedes the necessity of referring to the memoir presented to you. Lastly, he has taken the greatest care, in this paper, to omit entirely those assertions which have rendered his name popular, by becoming the subject of warm discussion, among people of all ranks, and he has confined himself strictly to his anatomical observations. Whatever, therefore, your judgment may be, no conclusion can be drawn from it, concerning a doctrine which has only a very distant relation to anatomy.

The consideration of the importance of the functions of the nervous system, and of the ignorance which still exists, concerning several points of its structure, notwithstanding the numerous examinations to which it has been subjected, superadded to these motives fixed our determination. Whoever flatters himself that he is able to throw any light on a subject so interesting and so obscure, is, in fact, entitled to be heard with attention, by a body such as ours; and we would be wanting in our first duty, if our examinations were not conducted with the greatest attention and strictest impartiality.

Forgetting, therefore, entirely what has been said and written for and against Dr. Gall, in public, in journals, or in pamphlets, not even confining ourselves solely to his memoir, which has not appeared to us to be written with all the order and perspicuity to be desired, we invited him, as well as Dr. Spurzheim, to our conferences. They readily dissected the brain in our presence, and we dissected it before them; we afterwards re-examined, by ourselves, the observations they communicated to us; lastly, we endeavoured for a time to adopt their manner of viewing the subject, and to give a clear and precise abstract of it, which we submitted to them, that they might be satisfied whether we rightly comprehended their ideas.

After taking all these precautions, we endeavoured to ascertain what was new or just in their ideas, and in the consequences which the authors deduced from them.

Experience very soon showed the brain to be the material instrument of the mind, and the essential organ of animal life, as well as that the whole nervous system performs a very considerable part in the functions of animal life. It is, therefore, not astonishing that physicians, anatomists, and phylosophers, in all ages, have investigated, with equal ardour, an organ of so much importance. It is with its study, that the history of anatomy commences and terminates. Democritus and Anaxgoras dissected the brain almost 3000 years ago, and Haller and Vicqd'Azyr, and twenty living anatomists, have dissected it in our own times; but strange to tell, there are not one of them who has not left discoveries to be made by his successors.

Undoubtedly we cannot expect a physiological explanation of the action of the brain in animal life, like that of the other viscera. In these the causes and effects are of the same kind.—When the heart causes the blood to circulate, it is one motion which produces another motion: when the stomach converts the food into chyle, it is the heat, moisture, gastric, and slow comprehension of its muscular coat, which unite to produce, at the same time, a solution and trituration, greater or less, according to the species of animal, and the nature of its food.

The functions of the brain are of a totally different order. They consist in receiving, by means of the nerves, and in transmitting immediately to the mind, the impression of the senses; in preserving the traces of these impressions, and in reproducing them with greater or less promptitude, distinctness, and abundance, when the mind requires them for its operations, and when the laws of the association of ideas recall them; lastly, in transmitting to the muscles, always by means of the nerves, the desires of the will.

Now these three functions suppose the mutual, but always incomprehensible influence of the divisible matter and the indivisible mind (moi) ; a hiatus in the system of our idea never to be supplied, an eternal stumbling-block of all our philosophies. But there is another difficulty not necessarily connected with the former. We not only do not comprehend, and never shall be able to comprehend how any traces impressed on our brain can be perceived by the mind, and produce images in it ; but however delicate our researches, these traces are in no way visible to our eyes, and their nature is perfectly unknown to us ; although the effect of age and of disease upon the memory does not permit us to doubt of their existence, or of their seat.

It seemed, at least, that the action of the nervous system upon the organic life, would be more easily explained, as it is entirely physical, and we might expect by means of investigation, to discover in this system some texture, some intermixture, or direction of parts which would render it more or less analogous to the vascular or secreting organs. There was especially no reason to doubt, that it would be possible to unfold their different portions, to assign their connections, their relations, and respective terminations, as easily as in the other systems.

This, however, has not happened. The texture of the brain, of the spinal marrow, and of the nerves, is so fine, and so soft, that all that has been hitherto said of them is blended with conjectures and hypothesis ; and the different masses which compose the brain are so thick, and have so little consistence, that the greatest dexterity is required to show all the parts of their structure.

In short, none of those who have examined the brain, have succeeded in establishing a rational and positive relation between the structrue of that organ and its functions, even those which are most evidently physical : the discoveries hitherto made known with regard to its anatomy, are confined to some circumstances regarding the form, connec-

tions, or texture of its parts which had escaped the observation of preceding anatomists; and whenever any one has supposed that he had proceeded farther, he has only introduced, between the well known structure and its commom effects, some hypothesis, scarcely capable of satisfying for a moment even the least sceptical minds.

All the actual discoveries which have hitherto been made, may be reduced to new methods of dissecting the brain, new connections and directions perceived between its different masses, and the organic elements composing them, or new peculiarities observed in some of its parts.

We are far, however, from despising these results; they direct us to the only route which may at last conduct us farther; and although we do not yet know the whole length of this route, we are satisfied, that every step made in it brings us nearer to its termintion, by some fraction of its strength.

We proceed, then, to explain and examine, under these three heads, of method, connection, and peculiarities, the discoveries announced by Drs. Gall and Spurzheim.

Anatomists know that there are three principal methods of demonstrating the brain.

The one most generally practised in the schools, and described in books, is that of Vesalius, which consists in removing successive layers of this organ, and in pointing out what presents at every cut. It is the one the most easily performed for the sake of demonstration, but the most difficult to be followed in the mind. The true relation of those parts, which we see always cut, escapes, not only the pupil, but the teacher slso; it is almost as if one were to divide the trunk of the body by successive sections to point out the position and form of the lungs, heart, stomach, &c. This method, however, is still almost the only one which is employed in the work of Vicq-d'Azyr, the most splendid, and one of the most vlauable which has appeared on the subject of the brain.

The second mode, which deranges much less the organ, the structure of which it is intended to explain, is that of

Willis, which, as far as we can judge by the obscure description of Galen, very much resembles that employed by the ancients. After removing the *pia mater*, the posterior lobes of the brain are raised, we penetrate between the *tubercula quadragemina* and the *fornix*, and cut its anterior pillar; after detaching the lateral parts of the hemispheres, they are turned forwards: in this manner we have a good view of the under part of the *fornix* and *corpus callosum*: we preserve entire, the great and small tubercles of the interior; but the thickness of the hemispheres renders it more difficult to practise this mode in man than in other animals.

The third method was very long ago sketched by Varolius, and afterwards more fully detailed by Vieussens. By this mode the under part of the brain is first examined, and the *medulla oblongata* is followed across the *pons varolii*, the *thalami optici*, and *corpora striata*; its fibres are seen expanded to form the hemispheres; it is even possible to stretch out the hemispheres by removing their lateral attachment to the crura cerebri; to divide longitudinally the spinal marrow and the cerebellum; and then each half of the former is seen forming a pedicle, which is implanted into the side of the hemisphere, as the stem of a mushroom is fixed into its *pileus*.

This mode possesses the very great advantage of allowing us to follow more easily the direction of the medullary fibres, the only circumstance which can afford us any idea with regard to the course of the cerebral functions; and it is probable, that it would have been more generally adopted, if Varolius had not represented it in a very coarse figure, and if the work of Vieussens had not always remained, for what reason it is difficult to determine, in a kind of discredit which it did not deserve.

It is nearly this method of Varolius which is followed by Drs. Gall and Spurzheim, and which a part of their memoir is intended to defend; a labour, indeed, very useless, for so complicated an organ as the brain, ought to be examined on every side; it must be cut into every way; and when-

ever any plan is discovered which points out some new circumstance, it furnishes an important addition to anatomy.

We shall therefore judge of their mode by the results; and, for that purpose, we shall detail and compare them with those which have been obtained by preceding anatomists.

It is well known, that the opinion most generally received with regard to the minute organization of the brain, is, that the cortical substance of the hemispheres, and of the cerebellum, being almost entirely vascular, is a sort of secreting organ: that the medullary substance, almost every where of a fibrous appearance, is a collection of excreting vessels, or at least of conducting filaments; that all the nerves are emanations from this substance, bundles of these vessels; that the *medulla oblongata*, and spinal marrow, is itself a bundle larger than the others, from which the different pairs of spinal nerves go off in succession: lastly, that the nerves called cerebral, are the first which go off from the great medullary mass of the encephalon; consequently, it is believed that all the influence of the nervous system upon organic life, as well as all the efforts of the will, descend from the brain along the nerves; and that the impressions received from the external senses, re-ascend by the same route; but by a singular contradiction, at the same time that they make the medullary substance, and consequently the nerves, originally extend to the whole of the cortical substance, several physiologists think themselves obliged to search for some circumscribed place from which all the nerves emanate, or, what signifies the same thing, in which all the nerves terminate; in short, for what anatomists call the *seat of the soul.*

It must be allowed, that this has been for a very long time the opinion most generally received, and that it is still very prevalent; although the most prudent philosophers have never brought it forward but as an hypothesis very slightly supported by facts.

Several of its partizans, however, have allowed themselves

to be led into doubts and contradictions. Haller, for example, says in one place, that he cannot suppose that medullary fibres originate any where else but in the brain;* in another, that every nerve comes ultimately from the medulla of the brain or cerebellum;† while, in a third place,‡ he supposes that the cineritious matter of the spinal marrow may form them in the same manner as that of the brain.

In fact, this distribution of the cineritious matter in different parts of the system, was one strong argument against the exclusive importance ascribed to the encephalon, and to it might be added several others.

It might be remarked at all times, that the action of the nerves upon organic life continues for some time after the brain no longer contributes to it. Well known experiments upon insects and worms proved, that if in man, and the other animals where the brain is very large, this organ is necessary to the functions of animal life, it is not always so in those species where it is of less size, and that in some of these we may even instantaneously produce, by dividing it, two centres of volition and sensation.

It has also been known for a very long period, that the spinal marrow does not diminish in proportion to the nerves which go off from it, as it ought to do if it were only a bundle of these nerves sent from the brain; that, on the contrary, it swells at certain places, from whence the largest nerves go off. Prof. Soemmerring has recently remarked, that the size of the *medulla oblongata* in animals is not in proportion to that of the brain as it ought to be, if it were a congeries of excretory tubes from the brain, but that, on the contrary, it is often in an inverse proportion; the successive investigations of Monro, Prochaska, Reil, have given us ideas of the structure of the nerves quite different from those which we would be obliged to form if we were to derive all of them from the medullary substance of the encephalon, and by it from the cortical substance. Several

* Phys. iv. p. 885. † Ibid. p. 393. ‡ Ibid. p. 384.

physiologists, therefore, within these few years, have again begun to consider the nervous system as a net-work, all the portions of which contribute, in a certain degree, and especially according to their size, to the organization and functions of the whole; and not as a tree which, having only one trunk, distributes itself into branches and twigs, after the manner, for example, of the arterial system.

Drs. Gall and Spurzheim, in adopting this opinion, do not adduce new proofs of it, but only repeat those which we have stated, and which had been brought forward many years before.

It appears, that in Germany and other places, various objections have been made to their system which we would not have made, but which they have taken the trouble to answer.

When they, for example, described, that in the fœtus without a head, the nervous system performs its functions of organic life without the assistance of the brain, it was opposed to them, that the acephali are only fœtuses where the brain has been destroyed by the effects of dropsy. This objection, which is true in some acephali, certainly does not apply to all; and it is by no means rare to see some which have been completely developed, although they do not show the smallest mark of having ever had either a head or any of the upper parts of the trunk.

We, therefore, readily agree with Drs. Gall and Spurzheim, with regard to the general idea which they entertain, as well as a great many anatomists, of the nervous system. But while they, with so many others, consider it as a net-work, they have some particular ideas concerning the links and knots of which this net-work is composed; and here the part of their doctrine begins which is peculiar to them.

As far as we have been able to understand, it appears to us, that it may be reduced to the following heads, or propositions:

1. The cineritious matter is the *matrix of the medullary filamenfits*; wherever it exists these filaments are produced;

it exists wherever it is produced. Whenever a medullary bundle crosses cineritious matter, it is enlarged by filaments received from the latter, and none of these bundles enlarge without the concurrence of this matter, whether it form a sensible enlargement, or only follow and accompany the medullary bundle.

2. The spinal marrow is not a bundle of nerves descending from the brain. The spinal nerves rise by filaments, some of which ascend, others descend; this is observed especially in brute animals. The cineritious matter of the internal part of the spinal marrow is the matrix of these filaments; the spinal marrow swells whenever it gives off a pair of nerves, and the more the larger these nerves are. Thus the spinal marrow of the larger animals, as well as that of insects and worms with red blood, is only a series of ganglia, which give off nerves; but all these ganglia communicate with each other.

3. The nerves generally called cerebral, and which arise from the under part of the encephalon, and chiefly from the medulla oblongata, do not come from the brain any more than the others; on the contrary, when they follow separately the roots of each of these into the substance of the medulla oblongata, we see that they re-ascend from the medulla towards the point where they appear externally, and that they do not descend from the brain, to pass through the medulla oblongata.

4. The brain and cerebellum are themselves only developements of bundles which have come from the medulla oblongata, in the same manner as the nerves proceed from it.

The brain, in particular, arises principally from the bundles called pyramidal eminences, which cross each other in passing out of the medulla oblongata, each going to the opposite side to that from which it comes: they swell out first in crossing the *pons varolii*, again in passing over the tubercles called *thalami optici*, and a third time in those tubercles called *corpora striata*, always from the addition of medullary filaments, which the cineritious matter, contained

in these three parts, adds to those they originally consisted of. The cerebellum comes from bundles called *processus cerebelli ad medullam*, or *corpora restiformia*, which receive an addition of filaments once only from the cineritious matter of what is called *corpus ciliare*.

5. These two pairs of bundles, after receiving these additions, after having assumed consequently a diverging direction, terminate each in two large expansions, covered externally by cineritious matter, which only here deserves the name of *cortical* substance; and these expansions folded in various ways, form what are called the hemispheres of the brain, the lobes, and the *vermiform prosesses* of the cerebellum.

6. From the whole extent of these expansions, other medullary filaments are produced, which converge from the two sides of the brain and cerebellum, towards the middle line, where the filaments of the one side unite with those of the other, and form what are called the *commissures*.

The corpus callosum, the fornix and its appendages, form the largest of the commissures of the brain; the anterior commissure is that which joins the middle lobes. The commissure of the cerebellum is compossed of the transverse layers of the pons varolii.

7. When we remove or lacerate the converging fibres which go to the corpus collosum, and which serve as a ceiling to the lateral ventricles, there only remains, under the cineritious substance, a medullary part lining it, and following all its folds; and far from forming a solid mass, as has been hitherto supposed, there is always in the middle of each evolution of the brain and cerebellum a solution of continuity, and by employing proper care, we may unfold this portion of the medulla, as we would unfold the cineritious substance if it were alone. In a word, each convolution is a kind of small purse or canal, closed externally by a double layer of cineritious and medullary matter, and on the side of the ventrical by the converging medullary fibres.

8. As the pairs of bundles forming the brain and cere-

bellum have their commissures, those which form the nerves have often theirs also, which may be very easily demonstrated in the second, the fourth, fifth, and seventh pairs, and very probably in the others.

9. The ganglia spread over the whole body are small masses of cineritious matter, which certain nerves, pass through and where they are strengthened as the pedunculi of the brain are increased in the thalami optici and corpora striata.

These two pairs of tubercles then are true ganglia to these pedunculi. The cineritious matter of the cortical part of the brain and cerebellum may likewise be regarded as a ganglion of the commissures, or converging fibres. That of the inner part of the spinal marrow forms, in like manner, the primary ganglia of the spinal nerves. The cerebral nerves have probably each of them a particular ganglion, and it is easy to demonstrate it in several of them. The cineritious matter, and consequently the ganglia, may therefore be compared to the mucos expansion which covers all the extremities of the nerves of the skin, of the intestines, and even the pulp of the labyrinth, and the sort of mucous varnish which covers the retina.

10. From these nine articles, all merely anatomical, all more or less susceptible of being verified, a tenth results, which completes, and is the essential character of the anatomical doctrine of Drs. Gall and Spurzheim, viz. that each pair of nerves forms a particular system; that all these systems communicate together, and re-unite in the great cord of the medulla oblongata and spinal marrow; and lastly, that the brain and cerebellum, far from being the origin or source of this cord, are, on the contrary, an appendix, or sort of *diverticulum*, reserved for certain functions, but receiving an influence from all the parts of the cord, and exercising one upon them by means of their communications.

We do not think that any anatomist can perceive any obscurity in this new explanation of the ten principal articles premised by the authors of the memoir which we are examining; they have, besides, acknowledged them to be the true expressions of their opinion

It only remains for us, therefore, to say, how far they appear to us to be true and new; this we shall do separately with respect to each of them, and with the more interest, as their will result from our examination a kind of treatise, in which the structure of the brain will be considered under different views, more or less important, and abounding with extensive consequences.

But, before proceeding to this, justice requires us to repeat the declartion of Drs. Gall and Spurzheim, that they do not pretend to have discovered many new facts; but that the chief merit which they claim consists in the connection which they have been the first to establish between the known facts, and in the general conclusions which they have deduced from them.

The first article which ascribes as a function to the cineritious substance the giving rise to the medullary filaments, or, as the authors of the memoir express it, the being the *matrix of the nerves*, is, indeed, only another expression for the opinion generally received. Disputes have existed with regard to the texture of that substance. Malpighi supposed it to be formed of small follicles; Ruisch, perhaps with more justice, only admitted a vascular net-work; others are of opinion, that there is also, besides the vessels, a particular substance; but it has been almost always agreed to regard it as a secreting organ, and the fibres of the medullary substance as the excreting organs of the substance which it secretes; it was therefore necessary that these fibres should arise from the cineritious matter. The physiologists who do not believe the nerves to be hollow, but suppose that they have the power of conveying a fluid in the manner that the metals conduct electricity, do not all deny, on that account, that the nerves receive their fluid in the cineritious substance; hence they also think that they arise from it. Those, lastly, who ascribe to all the portions of the medullary matter a secreting power, do not pretend to deny, what the eye demonstrates, the close adhesion of the medullary to the cine-

ritious matter of the cortical part of the hemispheres, and the immense quantity of filaments which go out as so many radicles, the cineritious portions of the corpora striata and thalami optici, &c.

Our authors have, therefore, nothing particular in the function which they ascribe to the cineritious substance. Even in generalising this function to all the parts of this substance, they only express, in more positive terms, what, as we have seen above, Haller suspected to be the case with regard to the cineritious portion of the spinal marrow.

Since this opinion is admitted by so many anatomists, there must be powerful reasons for it; in fact, besides what the eye informs us of the intimate connection of the two substances, the number of arteries which go to the cineritious substance, and which seem almost entirely to compose it, can have scarcely any other object than a copious secretion.

The quantity of cineritious matter dispersed over all the parts of the nervous system, and to which the authors of the memoir have the merit of recalling our attention, would, perhaps, explain sufficiently the functions which the parts of the system exercise without the concurrence of the brain, and would dispense with the necessity of having recourse to a particular power of secretion in the medullary substance, or even in the sheath of the nerve, as Reil supposes.

The *second article* draws a comparison between the spinal marrow of the superior animals, and that of insects and articulated worms, or those with red blood.

It is well known that, in the two latter classes, the brain is scarcely larger than the ganglia, or knots of the spinal marrow from each of which the pairs of nerves go off; that it is from the size of these ganglia, and from their separation, as well as from the smallness of the brain, that we endeavour to explain the divisibility of the individual, which is remarked in all these species, at least for some minutes, and which goes so far in some, such as the *earth-worms* and the *naides*, as to make two lasting individuals from one, by means of division.

Nothing resembling this had been perceived in man, whose spinal marrow had no sensible contraction, and only swells out at those places from which it gives off nerves to the arms and thighs; but Drs. Gall and Spurzheim have showed us a preparation of the spinal marrow of a calf, in which is observed a kind of slight swelling between each pair of nerves. It would be curious to ascertain exactly in what animals this structure is found, and if it has any relation with the power of executing certain voluntary actions, without the brain; if the turtles, for example, which live and walk several months successively without this viscus, have the spinal marrow more knotty than other animals with red blood, &c.

One of our number has begun inquiries with this intention, which have not afforded him sufficient results to be laid before this class; but he has already ascertained that there are no sensible knots in quadrupeds even very nearly allied to the calf.

The *third article* is subdivided, for examination, into as many heads as there are pairs of nerves.

The general result which the authors propose to prove is, that all the nerves arise from the medulla oblongata or spinal marrow, and not from the brain.

There is no difficulty as to the spinal nerves, which are only conjectured to come from the brain, but the roots of which undoubtedly no human eye can trace into it, nor even perceive a tendency to run into it.

There can be no more with regard to the last pairs of the encephalon, counting from the par vagum, and under it, for they arise by transverse filaments, as the spinal nerves, although they have not two bundles; and no anatomist has observed these filaments reflected towards the brain after having entered the spinal marrow.

There is still less doubt with regard to the nervus accessorius of Willis, which evidently re-ascends.

We have only, therefore, to attend to the first eight pairs, reckoning the facial nerve as a separate pair.

The *seventh* pair of Willis is, in fact, now generally acknowledged as forming two nerves, distinct at their origin as well as in their course. The soft portion, or the *auditory nerve*, arises transversely from the *corpus restiforme*, called also *processus cerebri ad medullam*. This nerve has been for a long time considered to be formed by the small white filaments traced over the ceiling of the fourth ventricle, and this is still the opinion of Haller,* of Vicq-d'Azyr,† and of Soemmerring.‡ However, as these filaments vary in number, and even in direction—as a part of them are sometimes seen re-ascending towards the corpus restiforme, or passing through it towards the pons varolii,§ and as it is not very rare to find them wanting, their continuation in the auditory nerve has begun to be doubted. Prochaska,‖ the two Wenzels,¶ &c. have formally denied it. The latter** and Dr. Gall have farther remarked, that these Striæ are generally wanting in animals.

The two Wenzels†† observed, in 1791, for the first time a small grey band slightly prominent, placed also transversely upon the corpus restiforme, and which constantly covers a part of the base of the auditory nerve, which it unites with the fourth ventricle. Prochaska is hitherto the only one who has given a representation of it.‡‡ It is likewise observed in animals; and Dr. Gall, who adopts, in this respect, the opinion of the Wenzels, remarks that it is the more swelled in every species the larger the ears are, and the more acute the hearing is.

In the *horse*, the *stag*, the *sheep*, it is a tubercle almost as large as the eminence called *testis*.

We have found this circumstance to be true.

* Phys. t. iv. p. 225.
† Explication des Planches, p. 95.
‡ De Frabrica Corp. Hum. t. iv. p. 256.
§ We have had a very marked example of the latter structure in the course of the researches occasioned by this memoir.
‖ Oper. Min. t. i. p. 338. ¶ Prod. p. 22.
** Prod. †† Ibid.
‡‡ Oper. Min. t. i. tab. iii. fig. 1.

It is besides evident, that if the origin formerly admitted were the true one, the auditory nerve would not, therefore, arise less transversely from the medulla oblongata, and that its visible roots would always come rather from below upwards than from above downwards.

The *facial nerve*, or portio dura of the seventh pair, and the *abductor* or the sixth pair, are then the first which can leave any doubt, whether they arise from the medulla oblongata, or from the brain, from behind or from before.

In man they both arise from the body of the medulla oblongata, immediately behind the posterior edge of the pons varolii, and so near to it, that several anatomists make them derive a part of their filaments from it.

The *facial* nerve, in particular, goes off some lines farther outwards than the other, at the angle formed by the pons varolii and corpus restiforme, at about a line from the point where the *auditory* nerve is detached from the corpus restiforme, which it had, as it were, surrounded.

The *abductor* seems to arise from the fissure which separates the pons varolii from the pyramidal eminences; and there are some anatomists who derive all its roots from the pons, others from the pyramids, others from both parts. There are others again who are not explicit with regard to this point.

According to the idea generally adopted, that the nerves descend from the brain, M. Soemmerring supposes* that the abductor nerve has its roots in the pedunculi, and that they are separate from them, turning backwards, after the pedunculi have crossed the pons varolii to form the pyramidal eminences. This is nearly the opinion of Vieussens, † but it is evidently the result of hypothetical reasonings, and not of actual observation.

To ascertain the true direction of the roots of these nerves, it is necessary to have recourse to the herbiferous animals, in which the pons varolii does not cover them, since it is not so large as in man.

* De Bas. Enceph. p. 140.
† Neurogr. Univ. p. 176.

This is what Drs. Gall and Spurzheim have done, and they found at once that the *abductor*, in them, arises some way behind the pons varolii, and appears to be the continuation of a small bundle which ascends between the corpus pyramidale and olivare. The filaments which produce it are longer at the back part and shorter before, so that they have, in miniature, the same arrangement as those of the accessorius of Willis. There is no reason, then, to suppose that it descends from the brain.

This observation concludes the discussion whether or not the nerve receives some filaments from the pons varolii; since it is only on account of the breadth of the pons in man that it approaches to its posterior edge.

We have not found any positive traces of this remark in the authors whom we have consulted, but we have convinced ourselves that it is true with respect to the herbiferous animals; and one of our number had even examined it some time ago in the horse. In carnivorous animals and the ape, the pons varolii and the sixth pair have a greater resemblance to what is observed in man.

With regard to the *facial nerve*, a transverse medullary band is seen in the herbiferous animals, behind the pons varolii, which begins exactly at the outer edge of the abductor, and passes over the root of the *par trigeminum*, or where it is continuous with the auditory nerve. The facial nerve appears to pierce the band obliquely from behind forwards. Thus it would seem to arise from the under part of the medulla oblongata, almost in the same manner as the auditory nerve arises from the upper part, and two pairs of nerves would be formed, the origin of which is, in fact, separated from each other by the whole thickness of the medulla oblongata, although they afterwards approach so as to touch each other.

Nor have we remarked that any author has pointed out this fact before Dr. Gall; but we are certain of its accuracy, and one of us had seen it, and delineated it some time ago, in the stag, horse, sheep, and rabbit.

The brute animals also show more clearly than the man the origin of the *par trigeminum*, or fifth pair.

It is generally said simply to arise from the lateral parts of the pons varolii, or from the extremity of the peduncali cerebelli. This is still adhered to by Vicq-d'Azyr* and Meckel.† Haller reckons this pair in the number of nerves, which may come both from the cerebrum and cerebellum.‡ It is, however, ascertained, that it neither comes from the one nor the other, and it may be traced deep into the medulla oblongata, to nearly an inch farther back than where it comes out.

Santorini asserts§ that he had traced the roots of it to above the corpora olivaria, and says, that it is more astonishing to see this nerve ascend from below, than the *accessorius of Willis ;* but he then supposes that a part of the fibres of the pedunculi which do not enter the corpora pyramidalia, they being, in fact, too small to contain them all, go on further, and then those, among others, are reflected which form this nerve ; a very gratuitous supposition, and which nothing sensible to the eye can justify.

M. Soemmerring seems not to have understood Santorini at the time he wrote this treatise *De basi encephali ;*‖ but he relates, that ¶ accident had made him afterwards trace the origin of this nerve into the substance of the medalla oblongata, even as far as the bottom of the fourth ventricle ; and, according to his favorite hypothesis, with regard to the seat of the soul, he conceives that the primary roots of it are bathed in the water of the ventricle.

Dr. Gall follows, in a steady and sure manner, this deep and low origin of the par trigeminum, as far as between the *corpora olivaria* and *restiformia.* He shows besides, that the breadth and thickness of the pons varolii in man, where the obstacles to its being discovered sooner. In fact, in the herbiferous animals, in which the pons is much narrower,

* Explication des planches, p. 52. † No. 46. et. 47.
‡ Phys. t. iv. p. 387. § Observat. Anatom. p. 64, 65.
‖ P. 135. ¶ De fabric. corp. hum. p. 212. No. 7.

the roots of the trigemini are easily traced, under a part of the pons, and under the transverse band placed behind, which we have seen to be partly the origin of the facial nerve as far as the longitudinal bundle which runs along the outside of the corpora olivaria.

We have confirmed these two observations; and in repeating the second in several animals, we have ascertained that it is found neither in the ape, nor in several of the carnivorous animals in which the nerves go off as in man; but always because the pons varolii in them also is broad. With regard to the first, it has appeared to us to be so certain, that we cannot help saying, that Vicq-d'Azyr has been deceived in deriving the roots of the fifth pair from the pedunculi cerebelli.* In examining always with the knife, he must have divided them, and lost sight of them too soon.

Every one knows that the *nervus patheticus*, or the fourth pair arises transversely from the valvula Vieussenii, behind the *testes*. There is nothing which could make it be derived from the great medullary mass of the brain.

The *motor oculi*, or third pair, comes from the pedunculus cerebri, towards its internal edge, where it touches the *perforated cineritious space*, lying between the two pedunculi and the two processus mamillares, and receives some filaments from it. In man its roots are arranged in a line, nearly in the direction of the pedunculi, the posterior roots are the longest, judging even by the external appearance; they proceed then rather from the posterior than the anterior part; but if a slight cut be made into the peduncle, this fact becomes still clearer. The greater part of these roots may be followed as far as under the pons varolii. A part of them is lost, or rather is formed around the black coloured part of the pedunculi. This argument is very well represented by Vicq-d'Azyr, *pl.* xxxi. *fig.* 2.

In other animals the roots a re more transverse, and more perpendicular; such, at least, we have observed them to be in the horse and the sheep.

* Mem. de l'Acad. des Sciences, 1781. p. 565.

The *optic nerve* is very generally considered as proceeding from the thalami optici; because its roots are spread out upon them in the manner of a thin membranous expansion, which almost entirely covers them. However, anatomists have not been wanting, who supposed that they could trace at least a great part of these, as far as the *nates;* such as Mongagni, Winslow, Zinn. Santorini* describes this origin very carefully, and adds another, which he makes to come from the testes; his pupil and editor, Girardi, confirms it,† Vicq-d'Azyr, who was also very well acquainted with these connections of the optic nerves with the tubercula quadrigemina, pretends, however, that they have likewise other roots in the substance of the thalami, which in the form of innumerable filaments, join the nerve in a great part of the course which it takes in surrounding the crus cerebri; and he boasted of this discovery.‡ But this appears to us to be a deception, into which he may have been led by his mode of making parallel incisions. It is very true, that a number of white filaments arise from the cineritious substance of the thalami; but they do not appear to us to go to the optic nerve. On the contrary, they are added to the bundle which comes from the pyramidal eminences, as we shall mention presently. Drs. Gall and Spurzheim have found, out a mode of dissection which demonstrates this very well, and which we shall mention again.

They suppose then that it is possible, at least in several animals, to remove from above the thalami, without affecting them, the medullary expansion of the roots of the optic nerve, and to follow them as far as the internal part of the nates, where they form a white lamina which occupies the centre of these tubercles.

The last point is certain; with respect to the first, as it can only be performed with the assistance of the handle of the scalpel, it is liable to the same doubt as all similar operations which may be tried on the brain.

* Observat. Anatom. p. 63. † Septemdec. tab. p. 34. Acad. de. Sc. 1783. p. 529.

Our anatomists farther remark, that in those individuals in which the optic nerve of one side is weakened and thinner, the corresponding tubercle is also smaller; and that in those species which have large optic nerves, the nates are of a larger size; but that frequetly in these cases the thalami are smaller.

The medullary tract which comes from the nates, meets in its course the tubercle called *corpus geniculatum externum*.

That which comes from the *testes* divides the first at the *corpus geniculatum internum*, and seems to slip under it, to cross it in passing forwards: thus Drs. Gall and Spurzheim do not believe that it belongs to the optic nerve; they have even for a long time thought that it forms the external root of the *olfactory*, which is indeed very nearly in that direction; but they have never been able to trace its continuity.

One of our members has made several researches on this part of the brain, which appears to have been only well known to Santorini and Vicq-d'Azyr, but which has never been properly delineated.

It is in fact, that in all quadrupeds, the principal bundle of the optic nerve goes from the nates to the *corpus geniculatum externum*.

It is also certain, that another bundle comes from the testes, which makes an angle with the first; and which, after swelling out to form the *corpus geniculatum internum*, seems to pass under the first bundle, and proceed further, but is soon lost to the eye and the knife.

Lastly, It is ascertained, that both the *testis* and the *corpus geniculatum internum*, are much larger in the carnivorous than in other animals; and this fact would be favourable to the idea, that they contribute to form the olfactory nerve, which is so conspicuous in that class.

But we think that we have seen in the ape, that the *corpus geniculatum internum* receives a bundle from the *nates*, as well as from the *testes*, and forms by their re-union a root of the nerve, which joins only very low down that which

comes as usual from the nates above the *thalamus opticus*. Drs. Gall and Spurzheim have themselves besides remarked, what one of us pointed out long ago, that the dolphins and porpoises, which have no olfactory nerve, have, however, very considerable *testes*.

These ainmals, likewise have the corpora striata as the others, which shows that these bodies do not possess the function attributed to them, of forming the olfactory nerve.

The first pair, then, will be the only one, the roots of which cannot yet be traced to the medulla oblongata, and which does not agree as yet clearly with the rule established in the memoir which we are examining.

Dr. Gall explains, conformably to the law mentioned in his first article, the enlargement of the optic nerves below their conjunction, by numerous filaments sent to them by the cineritious lamina interposed at the fore part of that conjunction; which filaments have been accurately described, and carefully delineated, by Vicq-d'Azyr.*

A strong objection was made to the origin, which our anatomists assign to the optic nerve, drawn from the structure of birds, which, it was said, have no *nates*, although their eye and optic nerves are of a very large size; but their answer is convincing. What Willis, Collins Haller, and other anatomists have called thalami optici in birds, are in fact the *nates*. The true thalami optici are situated before, with their third ventricle, the pedicles of the pineal gland, and the two commissures in their usual place; in a word, they are in every respect similar to those of the quadrupeds, in proportion to their relative size: the supposed thalami of Haller are, on the contrary, between the posterior and the commissure and the valvula Vieussenii; the aqueduct of Sylvius passing between them; it is with it that the ventricles communicate, which are peculiar to them in this class.

We have proved the truth of this important remark; it can admit of no reply. It is the more the duty of the reporter

* Mem. de l'Acad, 1783. p. 548. et. pl.xiii. fig. 1 et 2; and in his large work, pl. xxi. in all the figures.

to acknowledge it, as he had adopted the common error in his works.

Now, as the tubercles in question evidently give rise to to the optic nerves in birds, they confirm the origin which they ascribe to these nerves in the mammalia, and in the man, instead of invalidating it.

We may here repeat the elegant remark made by Vicq-d'Azyr, that these tubercles have a ventricle in birds in which the sense of sight is the most acute : as the olfactory nerves in the mammalia, in which the sense of smell is superior to the others.

We proceed to ARTITLE IV. in which our authors explain the relation between the medulla oblongata, and the brain, and the cerebellum.

The continuity of the medullary fibres of the pyramids across the pons varolii, with the crura cerebri, and these across the *thalami optici* and *corpora striata*, into the medullary substance of the hemispheres, was well known to Vieussens, who had also given the very just appellation of posterior corpora striata to the thalami optici ; but the figures,* in which he represents this important point of the anatomy of the brain, are very coarse; they only show simple filaments, which went on enlarging and seperating from each other, which is far from being the case.

This interesting point of view was afterwards almost entirely overlooked, because they contented themselves with cuts made in the superior part of the brain. Monro† and Vicq-d'Azyr‡ again introduced it; the latter especially represented this continuity in two very beautiful plates, although, perhaps still a little less exact than is necessary, because the preparer had not taken care to bend in his incision according to the direction of the filaments.

To these horizontal incisions already made by the three authors we have just quoted, Drs. Gall and Spurzheim add

* Neurogr. Univ. p. 38 et. 89.
‡ Nervous System, t. vii. fig. 1.
† Grand ouvrage sur le cerveau, pl. xxii. et xxiii.

a vertical one, which has the merit of explaining, according to their view, how these medullary bundles enlarge, and of pointing out the true termination of the filaments of the thalamus opticus, which Vicq-d'Azyr thought he had traced into the optic nerve.

This cut passes through the middle of the pyramidal eminence, of the crus cerebri, of the thalamus and corpus striatum of one side, obliquely forwards and outwards.

The bundles of the pyramids are there distinctly seen intermixing with those of the pons varolii, and with the cineritious substance which mixes there with them, and supplies some additions; passing from thence into the crus cerebri, they again receive filaments from the *processus cerebelli ad testes*. At one place, under the thalamus opticus, they unite into a white mass, to which innumerable filaments of the internal part of the thalamus are joined at acute angles, pointing forwards. It is essential to remark the latter circumstance; it proves that the thalami send their filaments forwards, and not backwards, as Vieussens had supposed; it points out also, that it is not into the optic nerve that these filaments go, as Vicq-d'Azyr thought.

The white mass then becomes stronger, and divides into a great number of diverging pillars, which constitute the white streaks of the middle of the corpora striata: the cineritious matter of the superior surface of these bodies gives also a great number of small filaments, as the thalami had given; at last, all these fibres are dispersed in the medullary mass of the hemispheres, where we shall follow them presently.

The two transverse whitish arches which are seen in the horizontal cut, and part of which Vicq-d'Azyr has expressed in his plate, are the places to which the greater number of filaments come from the superior parts of the thalami and corpora striata.

Such is the faithful description of what is perceived by the eye; one of us has delineated all this arrangement in man,

quadrupeds, and birds, in which the essential points are nearly the same.

We are aware that there is no better reason for saying that the great fibrous bundles go from the pyramids to the hemispheres, than from the hemispheres to the pyramids; since the nervous influence proceeds in both directions.

But one may and ought to inquire which direction the small fibres of the thalami and corpora striata take. Are they supplied by these tubercles to enlarge the great medullary bundle, or are they given off from the medullary bundle to sink into these tubercles? This latter opinion certainly could have no probability, and no one will condemn Drs. Gall and Spurzheim for adopting the opposite opinion.

They would then be in the right in this sense, when they say the medullary bundles go on always enlarging from the pyramids to the hemispheres.

But whence do the inferior extremities of the bundles, these same pyramidal eminences, come; or, whither do they go?

They cross each other about two fingers breadth behind the pons varolii, and immediately disappear behind that point, losing themselves on both sides in the two cords composing the inferior surface of the spinal marrow.

This is one of the most interesting facts in physiology and pathology.

Every body knows how frequent it is to see a paralysis of one side, occasioned by an injury of the opposite side of the brain; physicians of every age have endeavoured to explain this fact by a decussation which they vaguely supposed in the fibres of the brain, or in the deepest roots of the nerves.*

However, we see, almost every where, only transverse fibres, commissures, and not crossed fibres.

* Arétée de Caus. et Sig. Morb. lib. i. cap. 7. p. 34. B. edit. Lugd. Bat. 1731. Nervi ab initio enati protinus ad oppositos transeunt, se invicem permutantes in figuram literæ X.

There is only one place, at the posterior extremity of the medulla oblongata, which presents a true decussation, which was discovered by Domenico Mistichelli, and very well described by him in 1709.† François Pourfour du Petit‡ likewise described it the following year, and was the first who made it known in France.

How has it happened that a point of structure so evident, adopted by Winslow,§ Lieutaud,‖ Portal, distinctly described,¶ and plainly delineated,** by Santorini, should have been doubted by the great Haller,†† recently denied by very skilful men, and confounded by others, among whom may be reckoned Vicq-d'Azyr himself, with that of the transverse fibres which re-unite, in their whole length, the lateral parts of the medulla oblongata?

It is probably owing to the want of a sufficiently clear description, and perhaps, also, because the place of the decussation must be often cut in separating the head from the body.

It will be impossible to be mistaken in it, after the demonstrations of Drs. Gall and Spurzheim. When we separate from each other the two inferior cords of the medulla oblongata and spinal marrow, we see that they are separated by a pretty deep fissure, the bottom of which is occupied by transverse medullary filaments. This fissure is only interrupted at one place, which is what now engages our attention, and which is only two or three lines in length. The fibres of the pyramidal eminence of one side form there three or four filaments, which decussate upon the fissure with the opposite filaments, as the hairs of a mat, and which are blended afterwards with the rest of the medullary cord, into which they thus enter obliquely.

† Trattatte dell Apoplessia, Roma, 1709, in 4to.

‡ Lettre d'un Medicin des Hopitaux du Roi, p. 12. Namur, 1710, 4to.

§ Traité de la Téte, No. 110.

‖ Anatomie Historique et Pratique, t. i. p. 591.

¶ Santor. Observ. Anatom. p. 61. xii.

** Ibid. § xvii. tab. II. †† Phys. t. iv. p.

This decussation strikes the eye when we separate gently the edges of the longitudinal fissure of the medulla oblongata, because it is the only place where we cannot perceive the bottom of the fissure.

There is certainly some merit in having renewed the general knowledge of an important point of doctrine, which the doubt or denial of able men had caused to fall into oblivion.

Dr. Gall having established, as it appears, from this progression of medullary bundles of the brain across the pons varolii, the thalami, and corpora striata, his law of the increase of the medullary fibres by the cineritious substance, wishes to apply it to the cerebellum.

He has recourse here to the cineritious body of so singular a figure, which is found in the substance of the crus cerebelli, which has been called *corpus ciliare*, or *corpus fimbriatum;* the bundle called *processus cerebelli ad medullam* would give rise to the cerebellum, after having been reinforced by the corpus fimbriatum, as the pedunculi cerebri are by the thalami optici, and the cineritious part of the corpora striata. But perhaps this analogy is not complete. The corpus fimbriatum is enveloped, and, as it were, immersed in medullary matter, instead of giving a passage to it; and we do not observe that it supplies it with any filament.

Some person will perhaps add, after the example of Vicq-d'Azyr,* that the brute animals have no corpus fimbriatum; but the truth is, that it is only of a smaller size; and as their cerebellum is also much less, the fact would be more in favour than against the idea of our anatomists.

Articles fifth, sixth, and seventh should be examined together. These three form the most particular points of the doctrine of Drs. Gall and Spurzheim; the seventh especially, relating to the possibility of unfolding the brain like a membrane, has made the most noise in the world; but, as is too commonly the case, almost none of those who have spoken of it have properly understood our authors; and

* Acad. des Sciences, 1783, p. 471.

those who have thought that they had found the fact in more ancient authors, had still less understood the circumstance itself, and the passages where they thought they saw it expressed.

The terms in which we have expressed the ideas of Drs. Gall and Spurzheim, will make you already perceive that the question is not the unfolding the whole brain; they have expressly stated, at the meetings which we have had with them, that the parietes of the ventricles are such as they appear to be, and that they do not conceal any folds, except behind, towards the plaited margin, where their folds were, a long time ago, known to, and delineated by, Vicq-d'Azyr; only, our anatomists say these thick sides, formed by the converging fibres, are the only bands which retain the folds of the external substance; this is attached there as the folds of a plaiting, for example, are fixed to the stuff of a gown; if you remove the principal stuff, the folds will extend and form likewise a plain piece of stuff.

Your committee have examined, with all possible attention, the hemispheres of the brain, in order to judge of the truth of so new a doctrine.

They think that they can, in fact, distinguish two orders of fibres in the medullary matter, but they find it necessary still to confine very much the idea which might be formed of the unfolding, according to the expression which we have just stated.

We shall now explain, in succession, these two propositions.

When we trace with the knife the fibres which come from the crura cerebri across the thalami optici and corpora striata, we see that they intersect, at right angles more or less acute, those which go towards the middle line, and which form the corpus callosum and the fornix; it is even very easy to demonstrate their decussation in the inferior horn of the lateral ventricles; and the diverging fibres which come from the corpora striata, appear very evidently to form a layer ex-

ternal to the converging fibres which compose the corpus callosum.

But, does this external layer follow all the duplicatures of the still more external layer of cineritious matter, which is called cortical,—and does it unfold itself as the latter might be unfolded, if it were alone, and freed from all the white matter which fills it?

The one of these questions is evidently entirely independent of the other, and to be decided entirely by the testimony of the senses.

Taking, in the first place, the circumstance in the strict acceptation in which it seemed to be announced, we have used every effort to enable us to adopt or reject it with certainty; it would not be easy for us to make those who have not tried it understand what very great difficulties we have had to encounter. The medullary matter which fills the circumvolutions of the brain is so soft, that it sinks down by its own weight; however lightly one supports with the finger the convexity or the dorsum of one of these circumvolutions, its two sides separate horizontally, and each of them carry away a part of the medullary matter which filled up the interspace. The vessels are not ruptured, because they are for the most part placed in the direction in which the rupture takes place; and, besides, because they pass through this medullary matter on account of its softness, as threads would pass through jelly or pomatum. It appeared to us, then, impossible to prove that there was an actual solution of continuity; on the contrary, both with the naked eye and the magnifying glass, the two laminæ of the white matter appeared covered with small prominent points, with small filaments, which had quite the appearance of as many lacerations. We even tried to make the laceration begin so as to leave a thicker lamina of white matter on one side than the other; and it appeared to us that the separation was made as easily as in the middle.

The argument which the authors of the memoir draw from the example of *hydrocephalous patients* did not appear to us

much more conclusive. An accumulation of fluid in the ventricles of the brain may slowly distend the parietes, efface the projection of the circumvolutions, and render thin the medullary matter which surrounds them, without the latter being unnecessarily unfolded; the dropsy of the kidney distends and lessens the substance of that organ to such a degree, as to make it resemble a membrane, without any one being tempted to suppose that it was unfolded. The phenomena of hydrocephalous patients, who, having preserved their intellectual faculties for a long time, proves nothing further; for, as we do not know with what part of the brain, nor with what circumstance of its organization these faculties are connected, we can draw no conclusion from it relative to the essential structure of the brain.

Farther, we have also examined hydrocephalous patients; the parietes of the ventricles, though extended, had the same appearance as usual, and the circumvolutions, although thinned and partly effaced, nevertheless preserved their internal solidity.

Such were the ideas excited in us by the first memoir of Drs. Gall and Spurzheim, compared with the objects themselves; but these anatomists have since transmitted to us an additional note, in which they point out new modes of ascertaining the facts, and in which they explain their mode of viewing them with more precision.

M. Spurzheim has repeated in our presence these new experiments; vertical slices of the circumvolutions, macerated in nitric acid, diluted with rectified alcohol, became hard, and were divided more easily at the middle line; the same took place when they were boiled for twelve or firteen minutes in oil; when one blows on such a slice, or directs a small stream of water on it with a syringe, the separation may be made very easily in the middle, and scarcely at all at the sides. In the latter case, especially, the two surfaces which are separated, remain smooth, and the vessels passing through them untouched, without showing any traces of fibres which might have gone from the one side to the other.

These facts are accurate, but perhaps they only prove that there is less cohesion in the middle of a circumvolution than in the rest of its capacity, and not that it is formed of two laminæ simply applied, and not adhering together.

In other words, it may be admitted, in our opinion, that the white or intermediate portion of each circumvolution, is formed of two parts, which adhere to each other more slightly than the particles of each among themselves; or the union of them may be compared, for example, to that of the two laminæ of the dura mater, but not as was supposed, to that of the two sides of an intestine compressed, except, however, that the medium of union is not cellular matter, as in the dura mater; but the medullary substance itself a little softened.

Besides, as this is a point of fact, admitting an appeal to the senses, we do not pretend to give to our opinion more authority than it ought to have; this question cannot be long of being examined by every anatomist, and will find as many judges as observers; it cannot fail then of being soon finally fixed.

It is a great deal more difficult to demonstrate these orders of fibres in the cerebellum, than in the brain; and it is by analogy, rather than by actual knowledge, that Drs. Gall and Spurzheim admit their existence there.

With regard to what they say of the commissures of the brain, and cerebellum, their ideas are not new, or different from what has been already advanced by a very great number of anatomists; we may also add, that there is nothing in them which is not very probable.

We find the same probability in the commissures which *article eighth* attributes to each pair of nerves. It amounts almost to certainty in all the spinal nerves, whose commissures are the transverse filaments of the spinal marrow. We may suppose, that the small band which unites the two facial and the two auditory nerves in animals, is concealed in man by the pons varolii; the two pathetics touch each other on the valvula Vieussenii; the two optic nerves, as every one

knows, appear almost to be intermingled together anteriorly to the pituitary gland; besides, their roots must unite at the same time as the nates and testes on the aqueduct of Sylvius. There would then only remain the abductors, motores oculorum, and olfactory nerves, which would not have visible commissures. The anterior commissure of the brain also, is evidently united to the olfactory nerves in brute animals.

This generality of the commissures seems to assist in explaining the unity of action of the double organs.

The *ninth article* is one of those which have been the most contested by the anatomists of Germany, and which is in fact the most susceptible of being so. It establishes, in the first place, the generality of tubercles of cineritious matter for each pair of nerves; secondly, the analogy of these tubercles with those called ganglions; lastly, the analogy of these two kind of organs, both with the cortical matter of the brain, and with the mucous expansions of the organs of the senses.

That each pair of nerves belongs originally to some tubercle, or, at least, to some portion of cineritious matter of some form, may be very readily maintained, with regard to the spinal nerves, and as far up as the par vagum, since cineritious matter extends the whole length of the spinal marrow, although it is not possible to follow the roots of the nerves so far; it is even certain, with respect to the auditory nerve which arises from the small cineritious band in man, or from the much more marked tubercle which supplies its place in the greater number of brute animals; and with regard to the optic nerve, which has at least two of these tubercles, the nates, and the *corpus geniculatum externum*, and perhaps also two others, the testis and *corpus geniculatum internum;* the olfactory has at least one at the place where it rests upon the cribriform lamella of the ethmoid bone; but the eye can perceive nothing of a similar appearance in the other cerebral nerves in man, in the mammalia, and in birds, although the par trigeminum has a tubercle in fishes.

The probability of the analogy of the spinal ganglia, and of those dispersed in the nervous system of organic life, with

the portions of cineritious matter ascribed to the primitive origins of each pair of nerves, is liable to difficulties of a very different nature.

Anatomists, among whom it is sufficient to mention Winslow, have undoubtedly, for a very long time, regarded the ganglia as little brains, as sources of nervous action, independent of the great brain; others, such as Willis and Vieussens, have at least considered them as reservoirs of the animal spirits; or, like Lancisi, as organs which may be compared to a heart, and adapted to communicate a more rapid motion to these spirits.

Scarpa, of late, as well as Mekel and Zinn, in former times, was only willing to consider them as subdivisions, reunions, and re-compositions of nerves, enveloped and strengthened by cellular substance, bathed in a reddish fluid, and sometimes intermixed with fat.

The existence of this cellular membrane, and the fat which is sometimes deposited in them, have been acknowledged by the most distinguished anatomists of the present times. They are very distinctive characters, which do not permit the substance of the ganglia to be confounded with the cineritious matter of the brain. However, this substance has also some peculiarities, which ought not to allow it to be confounded with the ordinary cellular membrane; but the nature of these is certainly unknown.

The idea, that the ganglia dispersed between the different branches of the sympathetic nerves, have the effect of protecting the filaments of the nerves, reserved for the organic life, from the influence of the animal life, ought to occur, and, in fact, did early occur to physiologists; but why have not the spinal ganglia, which resemble the others so much, the same effect? Of this point we are still ignorant: all is here involved in darkness and obscurity.

It is of no service to science, to offer any new opinion, or revive any old opinion, without having more proofs in support of the one than of the other. It is better openly to confess our ignorance, and to separate distinctly the things which

are known from those which are not. The human mind, it is said, supports doubts with difficulty ; but it is precisely on that account, that the learning to bear with them ought to be one of the principal studies of men of true learning. The wokrs of some modern physiologists have led us to this short digression.

The analogy of the cineritious cortical part of the brain, and cerebellum, with the tubercles in its internal substance, such as the corpora striata, the thalami optici, the nates, &c. is much better established than that of the ganglia. Every one recognizes their nearly identity of substance ; sameness of function would there also be readily admitted. But what can we say of the comparison between it and the mucous body which lines the skin, and all its internal prolongations ? Here there can only be, with respect to structure, texture, in short, physical nature, a resemblance purely hypothetical. In absence of actual observation, it would require, then, to justify this comparison, some resemblance in the functions, in the uses, in the mode of existence during life ; but where shall we find these ?

We confess, also, that we do not comprehend the relation between the masses of cineritious matter, where the medullary bundles are strengthened in passing through them, and the rings which surround the base of the young branches of a tree ; in trees, the branches go off in sucession, one from the other ; but in the nervous system, the whole is formed at once. It is impossible to find here any thing but an accidental resemblance.

Such, gentlemen, is the roport which we have thought it our duty to make to you.

We have repeated all the observations of Drs. Gall and Spurzheim ; we have even submitted to a new examination, part of those which were made by older authors, but connected with theirs ; in fine, we have pointed out the degree of justice which we have found in the old as well as in the new remarks.

We, therefore, think that we have fulfilled, as far as lay

in our power, the commission with which the class has honoured us.

You will now perceive, that we are far from adopting all the views and all the observations related in the memoir of these anatomists, but that we are also far from rejecting them entirely.

Finally, it appears to us, 1st, That Drs. Gall and Spurzheim have the merit, not of having discovered, but of having recalled to the attention of physiologists, the continuity of the fibres which extend from the medula oblongata into the hemispheres, and into the cerebellum, which Vieussens first detailed, and the decussation of the filaments of the pyramidal eminences described by Mistichelli, by Francis Petit, and by Santorini, but with regard to which, some doubt had been entertained.

2d. That they are the first who have distinguished the two orders of fibres, of which the medullary matter of the hemispheres appears to be composed; the one of which diverges from the pedunculi, while the other converges towards the commissures.

3. That, by uniting their observations with those of their predecessors, they have made it very probable, that the nerves called cerebral ascend from the medulla oblongata, and do not descend from the brain; and that, in general, they have very much weakened, not to say overturned, the system which makes all the nerves come originally from the brain.

But it also appears to us, 1st, That they have generalized rather in an inconsiderate manner the resemblance of structure, and of functions in the various cineritious, or ash-coloured masses which are met with in the different parts of the nervous system.

2d. That the idea which they entertain of a solution of continuity in the middle of the medullary matter of each circumvolution, which would permit of its being unfoulded like a pipe or a purse, requires to be expressed in more definite terms than they have hitherto done, so as to indicate,

that there is no complete proof of an absolute solution of continuity, but only of a more feeble cohesion.

We ought, however, to remark, that these two articles do not affect their general result, with regard to the kind of separation and of reserve into which they place the brain; and we ought, at the same time, to allow physiologists and pathologists to judge how far this kind of separation or laying aside, which anatomy seems to point out, is justified by facts, and how far it may favour the explanation of the numerous and astonishing phenomena of organic and animal life, and especially of those in which these seem sometimes dependent upon, sometimes distinct from each other.

The entering into all these questions would engage us in endless discussions, and foreign to our commission.

Nor shall we propose to the class to decide upon the conclusion drawn by our anatomists, that there is no circumscribed place in the brain to which all the sensations go, and from which all the voluntary motions issue, but that both of these functions may be exercised in a greater or smaller extent of the nervous system.

This opinion is undoubtedly that of Haller, Bonetus, and of the greater number of physiologists. There can be no boubt that it is from having confounded the metaphysical simplicity of the roul with the physical simplicity attributed to atoms, that the desire has originated of placing the seat of the soul in an atom; but the connection of the soul and body being necessarily incomprehensible to our minds, the more or less limited sphere which it is wished to give to the sensorium would not, in the smallest degree, assist our conception of it.

But all these subjects are also too foreign to the business of the class; they are too slightly connected with physical facts; they lead to discussions too vague to form a proper object of attention for such a body as ours.

We think ourselves, however, obliged to finish our investigation by remarking, that, if we were even to adopt

the greater number of the ideas of Drs. Gall and Spurzheim, we would still be far from knowing the relations, the uses, and the connections of all the parts of the brain.

So long as we have not even a well grounded conjecture with regard to the functions of the pituitary gland, of the infundibulum, of the mammillary eminences, of the cords passing from these eminences into the substance of the thalami, of the pineal gland, and of its pedunculi, we must apprehend, that any system whatever, with regard to the functions of the brain, is very incomplete, since it will not embrace these parts so numerous, so considerable, and so intimately connected with the whole of this important organ.

This is almost finished with as much doubt, and as much uncertainty, as we began; but we can only require, on every subject, the degree of probability of which it admits; and the natural philosopher always performs his task very well, when he neither exaggerates, nor diminishes this probability, and when he fixes its degree with decision.

It is of importance to repeat once more, if it were only for the information of the public, that the anatomical questions with which we have been engaged in this report, have no immediate and necessary connection with the physiological doctrine taught by Dr. Gall, with regard to the functions and the influence of the relative size of the different parts of the brain, and that all that we have examined with respect to the structure of the encephalon might be equally true or false, without any conclusion being drawn for or against the doctrine, which can only be determined by quite different means.

Given at the Institute, the 15th April, 1808.

(Signed) Tenon, Portal, Sabatier, Pinel, Cuvier. The class approves of this report, and adopts its conclusions.

Conform to the extract,

(Signed) CUVIER, Perpetual Secretary.

It may justly be questioned whether indolence, or the fascination of great names, has contributed most to chain the minds of physicians to established systems ; for such is their servile attachment to the prevailing systems of nosology, that it becomes the duty of every man of observation, to detect their fallacies and refute their errors. If in diseases which are said to arise from specific contagion, (for want of an elementary acquaintance with their remote causes) and which exhibit symptoms the most uniform, we find such manifest abervations from written laws, what are we to expect from such as are occasioned by the sensible qualities of the air, the vicissitudes of climate, and the innumerable remote causes that never cease to operate till death deprives the body of the capacity of being acted upon?

In a paper published in the Medical Repository, I noted some of the appearances of the measles of 1802, and marked some of their deviations from the ordinary symptoms of morbillous fever. As this epidemic observes nearly equidistant periods of visitation, appearing about once in six years, no occasion of further observation presented itself, till 1808 ; when it manifested characteristics, not only at variance with the preceding epidemic, but which sat at naught all the laws of nosological classification. Having observed, for many years, that such of our great epidemics as pervaded an extensive surface of our continent, generally commenced their ravages to the eastward of Maryland, and travelled westward, and having ascertained that the measles were prevailing on the western shore of the Delaware, I determined to watch their progress and note their phenomena. Although my success has not been complete, my observations may lead some future observer to a more satisfactory result.

Whether the measles prevailed in Jersey, previous to January 1808, I have not ascertained. The first intimation of

their existence, was received from the opposite shore in the counties of Kent and Sussex, where they appeared early in January. About the middle of February, they began to appear sporadically in Caroline county, the eastern boundary of Maryland ; and before the middle of April, they had pervaded the whole peninsula. It appeared to me that the cause travelled one uniform pace under all circumstances of situation, wetsher temperature or population, and that the general cause that produced them in the first instance, and not the extension of the disease by contagion regulated their movements. This disease reached Baltimore early in May, and continued to prevail till the middle of June ; nor did it halt till its progress was arrested by the extreme heat of July and August, in the states of Virginia and North Carolina.

As the disease had appeared in 1802, it was to have been foreseen that it could not be generally epidemic, and that the children born since the last epidemic, would be the principal sufferers. This, however, was not the only cause, that diminished the numbers of sick. In many instances, only a solitary case occurred in a family where there were several children who had never been exposed to the causes. In one family only the eldest of ten children felt its influence ; and in many not half the children were affected, although they slept in the same bed, and lived in all respects alike. Several persons who escaped in 1802, were now affected. Among these were some adults, who suffered much more severely than children.

The type of this fever, was not only different from that of 1802, but from that described by the European writers. It did not assume the decidedly inflammatory character, so common to measles, but maintained almost invariably, a typhoid type. The pulses were seldom hard or tense. The affection of the pulmonary system was scarcely to be remarked, and neither the notery eye nor coryza were often met with. The atatck was seldom preceded by a chill, and the alternate

sensations of cold and heat were less remarkable than usual. After the first twenty four hours, the patient complained principally of a preternatural heat over the whole surface of the body and this sensation was often referred to the stomach, though in some cases this organ was entirely exempted from disease. A soreness of the throat was seldom complained of, and the raucedo voses so common in this disease, was scarcely perceptible. Neither pains of the head, back, or limbs, were much complained of. A dull heavy stupor reigned over the whole system, while a constant coma rendered the patient in a manner insensible to all passing events, except when the eyes were affected, as will be noticed in the sequel. The duration of the fever, as well as the period of eruption, constituted the most remarkable peculiarity of this disease. Instead of a continuance of the fever for a stated time, and the appearance of the eruption at the usual poriod, both were so irregular, that no rule could be established. Some patients laboured under a fever, for ten, twelve and even fifteen days, and convalesced without an eruption. In some the eruption appeared on the third or fourth day, but in others it did not appear till the seventh, tenth, eleventh, thirteenth, or fifteenth day, and in one case, an unremitting fever, continued till the twenty second day; when a copious eruption appeared, and the fever with the other symptoms, began to subside. Some of those who had suffered an attack of fever without the eruption were again attacked, and experienced a copious eruption. This was commonly after a lapse of twenty one days from the termination of the former attack. From this fact it may be questioned whether those who had the fever without the eruption, would be liable to the morbillous impression hereafter. Although some of them might have been rendered unsusceptible, it is most probable that the greater part of them, will be hereafter liable to the influence of the remote cause. The eruption seldom appeared complete in a few hours, or even in a day, but increased gradually from the slightest shade to a full eruption, proceeding by slow de-

grees, for five, seven, and sometimes ten days, before it was complete. In other cases it never was complete, but assumed an infinite variety of shades, from the faintest rubefacient subcuticular cloud, to the richest eruption. In a few cases, a pale crimson tinge appeared for a few hours, and vanished to be renewed again, at the intervals of three, five, nine, or eleven days, when it proceeded to a regulr crisis by a copious eruption. I ought to have semarked, that two persons were seldom attacked at the same time, in one family, but generally a second was taken sick on the twentieth or twenty first day from the indisposition of the first.

An hœmorrage from the nose sometimes preceded the fever, and generally on the day preceeding the attack ; but in other cases, it immediately preceded the eruption. Although the eyes were not affected in the usual manner, they suffered in a very extraordinary degree. In many cases the affection of one of them, was coeval with the fever, and after continuing for ten or twelve days, the other become similarly affected, and the inflammation of the first, gradually subsided. In some cases, the affection of the organs of vision, survived all the other symptoms, assumed a chronic form, and continued for many weeks, and sometimes for two or three months. In one case, a chronic fever of the eye, continued six months. The whole globe of the eye was interested, and generally much swollen and protruded, in the early stage of the fever. The vessels of the adnata, were deeply suffused with red blood. In many cases after the subsidence of inflammation, an opagne cloud obfuscated the cornea, and obstructed the rays of light, altogether, but generally only partially. During every stage of this affection, such was the sensibility of the eye to light, that a dark room, or a covering of black, was indispensible. Even after the disease assumed a chronic form, a strong light immediately occasioned a copious secretion of tears, attended with acute pain.

The CURE of this disease was not attended with much difficulty. The type was so mild and the local affections so

slight, that blood-letting was seldom required, unless it was indicated by the pain of the ball of the eye, and this was not the case in one of twenty of the number thus affected. Local bleeding by cupping was often very useful, and could it have been submitted to in every case where the eyes were concerned, would probably have been sufficient.—I regretted exceedingly the want of leeches, which promised every thing that could have been desired.—Blisters behind the ears were of essential service, especially where the discharge was kept up for several days.—As costiveness very frequently attended the inactive, recumbent state of the patient, gentle aperients become necessary. The sulphat of soda, or the acidulated tartrite of potash, were generally sufficient. As there were no increased morbid secretions of the stomach, intestines or liver, stronger cathartics were not required.—Emetics were of infinitely more use than in any other epidemic that has fallen under my notice. They were always useful as long as the heat of the stomach and skin were complained of, and generally afforded entire relief from these symptoms, although they seldom ejected from the stomach more than its natural secretion. Bile was seldom discharged, except after very frequently repeated or very violent efforts; and even then the portion was small, and its appearance natural. Emetics answered another valuable purpose. In cases where the eruption was retarded till after the usual time of appearance, they very often produced it in an hour or two after they ceased to operate, and put a period to the anxiety and restlessness that had been so distressing. This remedy did not occur to me in the beginning of the epidemic, but experience ere long taught me to rely on it as a safe and excellent practice.—It ought not to be omitted, that when the affections of the eyes remained after the patient had recovered from the other symptoms, small and long continued doses of the sub-muriate of mercury generally succeeded in removing them, and in many cases without the least affection of the salivary glands.—The only remaining remedy worthy of notice, is the application of *cold*. I had been, very early

in the disease, in the habit of bathing the eyes in cold water, sometimes in the form of ice, and with the happiest effects; but had not attended sufficiently to the temperature of the apartment. Although the cool regimen had all along been observed, the salutary effects of a lower temperature were not duly estimated, till Dr. Stevenson* called my attention to it. He informed me, "that the lowest degree that could be obtained at that season, was not only the most agreeable to the patient, but a most powerful mode of destroying the heat of the body so much complained of." During the first week of May, while the mercury of Farenheit varied from 50 to 55° in the shade, the windows were frequently thrown up, and the external air suffered to equalize the temperature of the room with that of the external air. The patient always expressed the greatest satisfaction, and the most striking proof of its utility was manifested by the removal of the coma and general stupor that attended all the strongest marked cases. The face and hands were often bathed with cold water, to the inexpressible delight of the sufferer. Of the great number of cases that fell to my lot, not one proved fatal.

* In a paper published in Barton's Journal, and the Baltimore Recorder, Dr. Stevenson insisted on the propriety and utility of the *cold* regimen; but many having considered it only as an encomium on the old doctrine of a cool regimen, have not been aware of its importance.

REVIEW.

An Inaugural Dissertation on the Medical Virtues of the White Oxyde of Bismuth.

By Samuel W. Moore, A. B. New-York, Printed by T. & J. Swords.

"——omnes boni medici ad ea remedia semper confugient, (sive "modus quo prosint intellectus fuerit necne) quæ certa expe- "rientia prodesse comprobaverit."

Greg. Consp. Med. Theoret.

ALTHOUGH the Magitery of Bismuth was introduced into practice by Dr. Odier of Geneva, in 1786, like opium, mercury, arsenic and every other of the most energetic remedies, it has been alternately neglected and revived by the practitioners of Europe. The volume of evidence in its favor, is however, now so full, that the proofs of its utility can be no longer resisted. Dr. Odier stated it to have been found in his hands " a powerful remedy in spasmodic pains of the stomach and bowels, particularly such as arose from organic debility or a relaxed and emaciated constitution."

Marcelot in his " Cours Elimentaire Theorique et pratique Pharmacie Chemique in 1803, after speaking of it aa a mineral but little used in medicine adds, " L'oxide de bismuth est employe, interieurement par qeulques praticiens, a la dose de 4 a 6 decigrammes, soit in pilules soit en poudre dans une boisson appropriee, dans les engorgmens lymphatiques."

Dr. Willich in his Domestic Encyclopedia remarks, " in medicine the calx and flowers of bismuth, were formerly used in cases where antimonial preparations are now employed with greater safety, and equal effect : so that the former are at present chiefly converted into pigments and cosmetics. Nevertheless, we are possessed of the most convincing proofs, that the magistery of bismuth is one of

the most powerful antispasmodics, especially in cramps of the stomach.

Dr. Marcet in his paper read before the London Medical Society, in 1801, but not published till 1804, states in a note " since the above paper was read before the society, I have had sufficient opportunities at Guy's Hospital, of trying the oxyde of bismuth, in spasmodic affections of the stomach, and those trials have fully confirmed the opinion which I gave three years ago on the utility of this medicine."

The Edinburgh Medical and Surgical Journal for October 1806, in the report of the Carey-street Dispensary, gives the following paragraph : " For the gastrodynia which is extremely frequent among the poor at all seasons, the oxyde of bismuth, which was recommended by Dr. Odier, of Geneva, has been employed both by my colleague Dr. Laird, and myself with considerable success. It has in some cases produced a permanent relief, after the usual tonics and stimulants had been taken with a trifling and but temporary advantage."

The London Medcial and Physical Journal for 1806, speaks in high terms of commendation of this medicine, " in dyspepsia, and views it is an important addition to the list of stomachics."

In the Journal de Medicine, Avril 1807, Mons Leroux extols the power of the oxide of bismuth, in affectibns of the stomach, and many other diseases, but does not specify the nature or characters of the other morbid states in which he had found it useful. He adds Mais jai ete oblige d'en porter le dose beaucoup plus haut qu'il n'est indiquè dans l'article qu'on vient de lire. J'en ai quelquefois donne chaque *jour jusq'a trente groins divises en trois doscs et je n'en jamais ivu resulter d'inconvenient.*"

Dr. Clark, in the Medical Report for Nottingham, published in the Edinburgh Medical and Surgical Journal, in 1808 and 1809, corroborates the preceding accounts by the most decided evidences of the virtues of the oxyde of bismuth.

Notwithstanding the mass of testimony from abroad in favor of this new addition to our pharmacopeias, it seems to have attracted but little of the attention of the faculty of the United States, till Dr. Moore reduced the scattered notices of its powers into one body in his dissertation. We abstract from this judicious essay as much of Dr. Moore's experience and reasoning, as the limits of our plan will admit.

New-York, August 1, 1810.

Case 1st. C. a mulatto woman, aged thirty-six, has, for the last seven months, been dyspeptic, during which time I have occasionally visited her and given her medicine; she is better, but still complains of being troubled every morning about four o'clock, while still in bed with sickness, preceded generally by coughing a few times, when she throws up from her stomach a watery fluid, seldom very acid. She assures me she has not been free from these attacks, scarcely for a single day during the last seven months. She has more than once taken emetics, and succeeded their use by gentian and orange-peel, without deriving from them much benefit.

Although I did not doubt that many cases might be found in which bismuth was calculated to do more good than in the present one; I nevertheless felt a desire to give it a trial, and accordingly directed her to take, three times a day, five grains of the oxyde,* with fifteen of gum-arabic and sugar.

September 5th. She is better, and tells me, she vomited but twice since she commenced taking the powders. She has taken seventeen doses of the powders.

* *Dr. Moore's mode of preparing the White Oxyde of Bismuth.*—The bismuth to be dissolved, should be previously reduced to powder in an iron mortar. Let three parts of nitric acid for one of bismuth be diluted with an equal weight of pure water. To this menstruum contained in a glass vessel, add the bismuth at intervals, and let it stand till it is all dissolved. Let the clear solution be decanted from the sediment, (a small quantity of which will always be found) and a few ounces of it be poured into a glass vessel capable of containing half as many gallons as there have been measured ounces put in; the vessel is then to be filled with pure water, when a copious and perfectly white precipitate will be instantaneously formed, giving to the liquid the appearance of milk.

9th. She has very little sickness, but still throws up in the morning a small quantity of fluid from her stomach. I gave her to-day some more of the oxyde, with directions to take it as before.

12th. Since she recommenced the use of the remedy, she has had no return of either sickness or vomiting.

New-York, September 9, 1810.

Case 2d. Mr. L. aged forty-five, has, for the last three years, been afflicted with cardialgia, attended by extreme flatulence; he has at no time during this period, been free from these complaints. He has frequent acid eructations,

After this has subsided, the clear fluid must be decanted, and fresh water thrown on the precipitate to wash it. This operation must be several times repeated, until no acid taste is discernible in the decanted water.

This precipitate, which is the true white oxyde of bismuth, should be suffered to dry without heat, or indeed light, for the attraction between bismuth and oxygene is so weak, that if the oxyde, while drying, be exposed either to a very moderate artificial heat, or the direct rays of the sun, it parts with a portion of its oxygene, and loses its whiteness.

The oxyde prepared in this way, is purer and whiter than that formed by any other process. I have precipitated it from its solution, both by potash and ammonia, but in neither case was it so pure a white; and indeed the administration of the oxyde, prepared by means of an alkali, might be attended with danger, in case the bismuth had been alloyed with other metals, which would also be thrown down by the addition of an alkali to the solution.

Whatever reason we may have for thinking the bismuth we make use of impure, (which can scarcely be the case without being discoverable from its appearance) still we have no reason to fear, that the oxyde will partake of the impurities of the metal which has been employed, if it be prepared in the manner which has been directed; for Nicholson assures us, "the precipitation of the nitrous solution by the addition of water, is the criterion by which bismuth is distinguished from all other metals.† And indeed, if our only object were to separate the bismuth from its alloys in a state of purity, no better means could be employed to obtain our end, than those which have been recommended for preparing the oxyde.‡

† Nicholson's Chem. Dict.

‡ No glazed earthen vessel should be employed, on account of the lead so frequently used in glazing them.

which he says are also very acrid, occasioning his throat to feel as though it had been scalded; sometimes also, though rarely, he is sick at his stomach when he vomits up a matter having the color of coffee-grounds. He has a number of times taken emetics, and since the commencement of his complaint, has been in the daily habit of taking magnesia; from the use of which, although he has sometimes thought it afforded him temporary relief, it is pretty evident, from the continuance of his disease, that very little permanent advantage has resulted.

During the last winter and spring, Mr. L. had occasional attacks of some gnawing pains at the pit of his stomach, which warm external applications would commonly in the course of fifteen or twenty minutes relieve. Throughout the summer, he has been generally free from those spasmodic pains; but for the last eight or ten days, they have attacked him much more severely, usually returning two or three times a day. These attacks are very violent, occasioning great anxiety, succeeded by insufferable pain, throwing him immediately into a profuse perspiration. The warm applications, from the use of which he had on former occasions found benefit, at this time afford him not the slightest relief.

For the last two months, he has been making use of very strong bitters, and I think it possible he has injured his stomach by the large and repeated doses he has taken. A very sparing use of either fermented or spirituous liquors never fails to increase his cordialgia. His bowels are quite regular and his pulse feeble.

Mr. L. sent for my father, who directed him to take three times a day, five grains of the oxyde of bismuth, combined with an equal weight of refined sugar, and twice its weight of starch.

September 11th. He has had no return of the pain in his stomach since he commenced the use of the oxyde, but feels well enough to-day to take a ride into the country.

Sept. 17th. He still continues free from the spasmodic pain of his stomach, his cardialgia, although not well, is

better than it has been hitherto. He has taken none of the bismuth since the morning of the 13th, when he took the twelfth and only remaining dose he had, and being out of town, he had it not in his power to procure another supply until his return to the city to-day.

Monday evening. He had this afternoon a return of that distressing anxiety which has always hitherto preceded his attacks of pain in the stomach. Fearing he was about to have another attack, and placing full reliance on the oxyde from his experience of its efficacy, he sent immediately for a supply of it; but before the person returned with the medicine, he had taken forty drops of laudanum; he however took directly a dose of the oxyde, and the pain did not, as he expected, succeed; but as he had taken laudanum previously, we should not be satisfied in attributing to the bismuth alone, the disappearance of his unpleasant symptoms.

Wednesday, Sept. 19th. Our patient has not since had any return of the spasm; the cardialgia is infinitely less troublesome. I gave him to-day, twenty-four doses of the powder, each containing six grains of the oxyde, directing him to take one three times a day.

Sunday evening, Sept. 23d. Mr. L. is now free from all complaint. His heart-burn, which was very troublesome till he began taking the bismuth, does not at present appear, unless it be brought on by the use of wine or brandy; he can however drink moderately of West-India spirits or gin, without experiencing the least inconvenience from so doing. Thursday last, after riding twelve or fourteen miles, he began to feel some pain in his stomach, but was immediately relieved by taking his customary dose of the oxyde.

New-York, Sept. 24, 1810.

Case 3d. Mr. B. the subject of the following case, has been for three or four years dyspeptic; during which time, he has almost continually suffered from cardialgia and pyrosis, and very frequently severe colic pains. The fluid which he threw up from his stomach, was, at times, he said, so

extremely acid, that it felt as if it were cutting his throat while passing through it. He has been long in the habit of using the alkaline solutions, absorbents, &c. but without effect.

Mr. B. has, at my request, very obligingly drawn up a statement of his own case, the substance of which I shall give nearly in his own words:

I have been for several years troubled with acidity in my stomach, a watery fluid running from my mouth, and an unpleasant sensation or pain at my breast, frequent colic pains; sometimes a cough, which was always accompanied by an unpleasant taste; a great heaviness and inclination to sleep, with an aversion to motion. My appetite has always been good, and sometimes indeed voracious; but even at these times my stomach would be often disgusted by the first mouthful of food I swallowed. When the heaviness came over me, I felt a sensible diminution of my strength and spirits. I have taken soda, magnesia, chalk, and rhubarb; but have found no relief from them. On the 16th of Sept. I called on Dr. Moore, and obtained from him a few powders, of which I have taken three a day, and have not, for the last five days, felt any symptoms of the complaint.

Sept. 24*th*, 1810.

Mr. B. took fourteen doses of those powders, containing altogether one dram and ten grains of the oxyde. He assures me, that he has never felt better than he does at present, and says, that if the powders produce a permanent cure, it will be one of the happiest events of his life.

Sept. 20th. I saw our patient this day, when he informed me of his having continued thus far perfectly free from all indisposition."

Dr. Moore adds, that "Dr. Stringham, of New-York, has lately administered the oxyde with astonishing relief in a case of menorhagia."

These facts are sufficient to evince the safety and utility of the internal use of the oxyde of bismuth, and leave us but

one point for doubt or apprehension. The tone of the stomach once impaired, is so predisposed to a repetition of the same morbid phenomena, that notwithstanding the temporary relief derived from various stimuli, relapses are very common; but we trust, that the candor of Dr. Moore and the experience of the faculty, will soon determine whether the oxyde of bismuth be a radical or palliative remedy.

We tender our acknowledgments to Dr. Moore, for the pleasure and instruction we have derived from his ingenuity and industry. We consider his dissertation as a plain, candid and discriminating exposition of his subject; very remarkable for that "lucidus ordo" preserved throughout, which falls to the lot of very few juvenile writers.

"Premordia vestra
Vix pauci meruere senes: metasque tonetis,
Ante, genas dulces quam flos juvenilis inumbret
Oraque ridenti lanugine vestiat ætas."

Dr. Daniel Drake, in a pamphlet modestly entitled "Notices of Cincinnati," has exhibited an accurate account of the Topography, Geology and Climate of this delightful region. To these he has addea, a Celendarium Floræ of the country. He promises us also "memoranda of diseases," from which we anticipate a rich repast. He concludes his researches, with the following description of what he has denominated, "A new disease," and which we are disposed to consider as one of the numerous family that are to be referred to miasmata for their remote causes.

In the spring of 1809, Dr. Barbee, of Virginia, on returning from a visit to the Madriver country, in this state, gave me some information respecting a new and formidable disease which had appeared among the settlers of that tract. Since that time, I have been able to collect several additional facts respecting it, from different persons, more especially Mr. William Snodgrass, and Mr. John M'Kag, two intelligent and respectable inhabitants of that country, who have several times experienced the disease in their persons and families. A summary of the whole, is here given, that physicians may determine how far it deserves the appellation of a new disease.

It almost invariably commences with general weakness, and lassitude, which increass in the most gradual manner. About the same time, or soon after, a dull pain, or rather soreness, begins to affect the calves of the legs, occasionally extending up to the thighs. The appetite becomes rather impaired, and in some cases nearly suspended; sensations of a disagreeable kind affecting the stomach: upon taking a little food, however, a greater disposition for it is generated, and more agreeable feelings are introduced throughout the whole system. Intestinal constipation in this, as in all the subsequent periods of the disease, exists in a very high degree. A strong propensity to sleep occurs, and according to Dr. Barbee, the pulse "is full, frequent, round, and *somewhat* tense, but regular." During this stage, exercise of any kind is highly detrimental, and

if persisted in, soon produces loathing and nausea at the stomach. If the patient repose, upon first experiencing these symptoms, they generally cease, and he is allowed a longer exemption from the *vomiting* that awaits him. Sooner or later, however, that symptom almost invariably succeeds the predisposition we have described, and either proves fatal in one, two, three, or more days, or leaves the patient in a most exhausted state, from which he recovers only to sustain, at no distant period, a repetition of the same attack.

The matter ejected is sometimes bilious, but much oftener sour, and so acrid, that its action on the throat, in one case, (which proved fatal) was likened to boiling water. Towards the close of mortal cases, it is occasionally very dark coloured, so that it has been compared to that very convenient and fashionable object of similitude—coffee-grounds. At this time the intestinal constipation is very great: Mr. Snodgrass knew one patient in whom it continued for nine days, throughout which he took no food whatever, and vomited during six of them. After such an attack, the propensity for sleep is destroyed, and an uncommon degree of watchfulness is produced. The patient remains languid, and his face and person generally become rather tumid. His skin is cool, palish, and frequently affected with clamminess. He has a disagreeable burning sensation in his stomach, and hot eructations are very troublesome. The thirst is considerable. The breath is peculiarly disgusting, even loathsome. The appetite is generally poor; and the inclination to costiveness remains. These symptoms often continue for several months, during which the patient experiences frequent returns of the vomiting. But at length, more especially upon the approach of winter, they gradually wear away, leaving the patient considerably worse than they found him, and liable to fresh attack the ensuing summer.

Nothing like *regular* periodical exascerbations are observable in this disease; no chilliness occurs; the colour of the

skin and eyes do not deviate widely from that of health, and give no striking indication of bile; there is no pain in the region of the liver, nor in the shoulder; it does not terminate in dropsy; nor are there any symptoms which bespeak it a disguised or anomalous intermittent. It however prevails (though not exclusively) in aguish situations, and intermitting diseases are thought to have declined since its appearance.

It affects all ages, conditions, and both sexes, indiscriminately; except probably very young children. They however are not wholly exempt from it. Emigrants are not peculiarly liable to it. It was first observed in the summer of 1806, and is thought annually to extend its geographical range, and to become more intense. It sometimes commences in July or before, but oftener in August, and continues till the approach of winter, when it generally, but not always subsides.

The cure of this disease seems hitherto to have been left chiefly to the people, who have not yet discovered any certain method. Purging was a remedy that naturaly suggested itself; and by some it had been thought very serviceable, more especially when effected by aloes; but others assert that they have frequently known a cathartic to increase the vomiting, and therefore rely more on enemata. All agree however, that the intestinal obstructions are to be overcome; and that the less the means made use of, affect the stomach, the better. Vomits evidently do harm. Blisters to the gastric region are considered the most efficient remedy, tonics have been used, but no great benefit appeared to arise from them. Wine and salted meats, however, have appeared to do good, and are relished beyond any thing else. Indeed, eating a little frequently, whether an inclination exist or not, has been found a good palliative: It relieves the stomach from the gnawing which so perpetually exists. Alkaline lee has been used in one case: it gave some temporary relief, but not more than almost any other substance which might be received into the stomach. Bleeding

has been occasionally resorted to, but with doubtful advantage. Ardent spirit appears to render the disease worse: It is not however, much sought after, all inclination for it, generally being destroyed. Tea and coffee, also, with several other articles of diet, which were agreeable before the disease, are in many cases disliked for a long time after.

This disease is unequivocally observed to effect four domestic animals; the horse, the cow, the sheep, and the dog. It is often fatal to the two former; but not so fatal to the latter. It as frequently attacks horses in the winter as summer, and sometimes kills them in twenty four hours.

It prevails chiefly in the neighbourhood of Staunton, on the great Miami, and in the country south of Madriver, between Dayton and Springfield. In those tracts, ponds, and marches occasionally occur, more especially in the former. The soil and water are calcareous. The timber generally oak.

At a commencement in the University of Pennsylvania, held on the 26th of April, 1811, the Degree of Doctor of Medicine, was conferred on the following gentlemen; citizens of the state of Maryland.

John Austin, John H. Briscoe, William Hammond, John D. Perkins, William Purnell, Robert J. H. Purnell, John D. Readel, James Stewart, and Matthew Vanlear.

ERRATA.—Page 24, fourth line from the bottom, a *comma* instead of a *period*, after the word *application*.

Page 83, tenth line from the top, instead of *observations*, read *aberrations*.

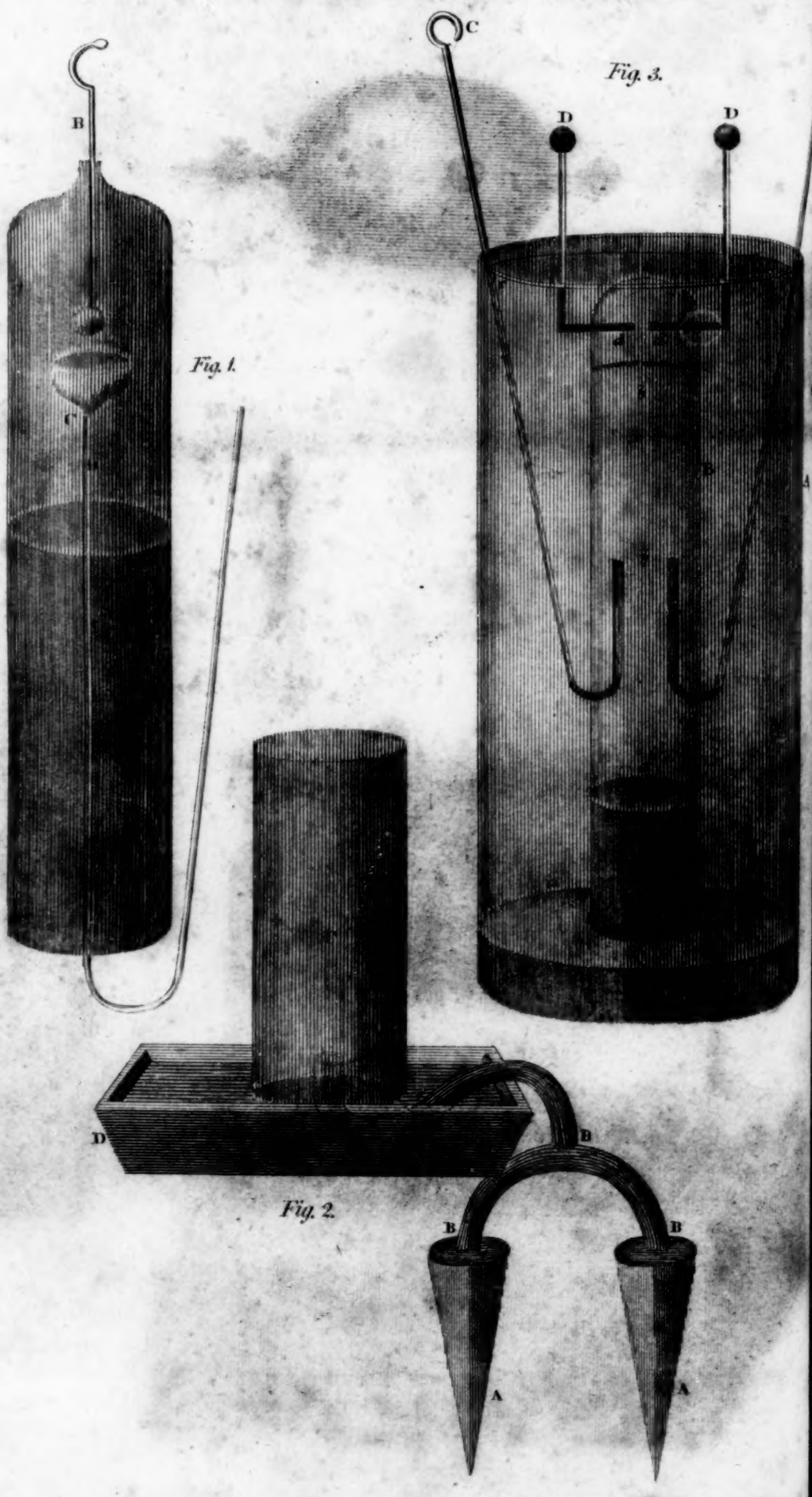
Page 85, seventh line from the top, instead of *voses*, read *vocis*.

Page 89, third line of the review, for *every*, read *many*.

Page 96, fourth line from the bottom, instead of *premordia*, read *primordia*.

Page 89, first line of the subject, instead of *Magitery*, read *Magistery*.

Professor Davy's electro-chemical apparatus



Professor Davy's electro-chemical apparatus.

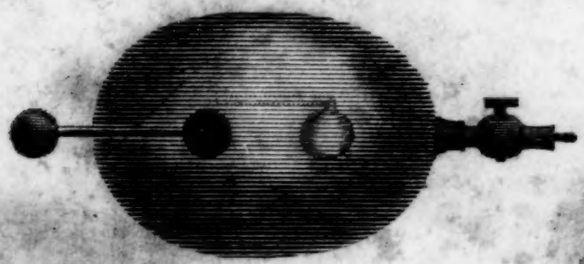


Fig. 5.

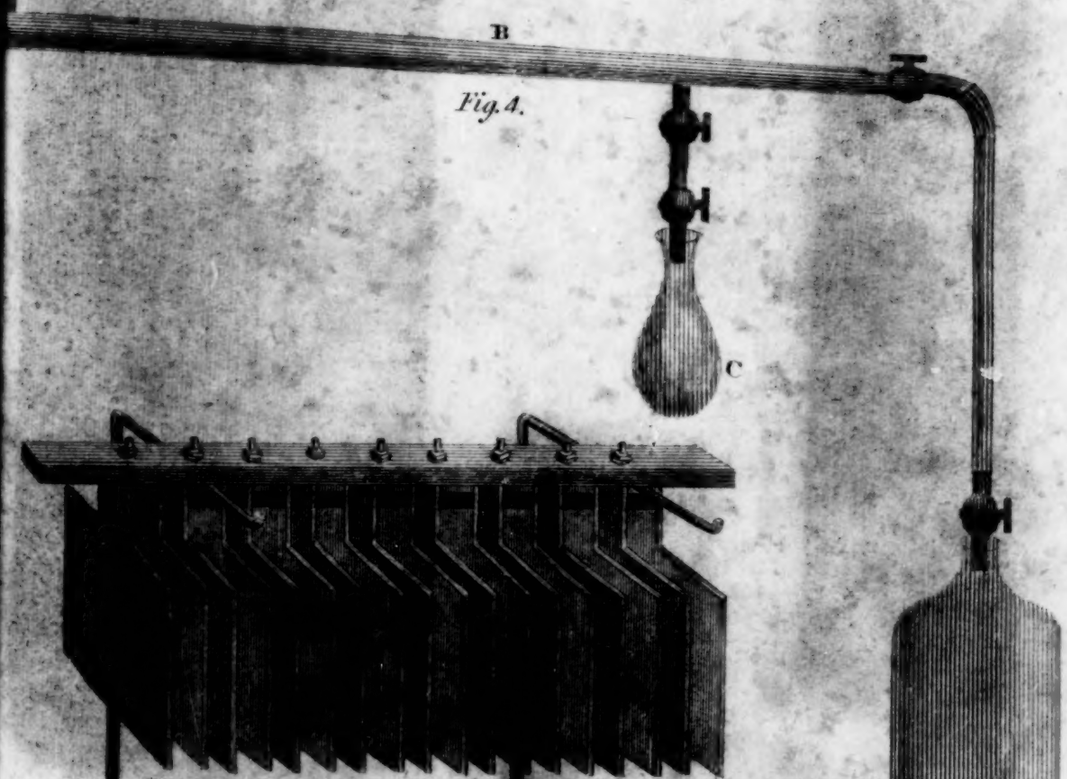
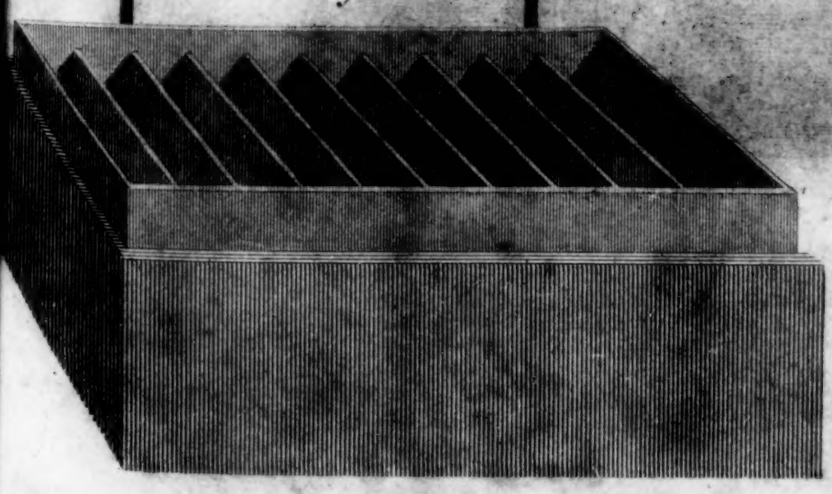


Fig. 4.

Fig. 6.



Kinsaer sc.